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ON TIDAL THEORY AND TIDAL PREDICTIONS.

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[*Concluded from Volume CXX., page 252.*]

Having in the above discussed the method of predicting the time of high and low water, we now proceed to the predictions of height.

As mentioned before, the zenith distance of the heavenly bodies from high and low water, at the time of the generating lunar transit, is introduced into the computations of height, in the place of their declination and rectascension (relative meridional position). For this purpose the following tables have been constructed:

Table 22.—Zenith Distances of Moon at the Time of the Generating Lunar Transit, from High Water on Parallel 40° North.

	Lunar Declination.	Zenith distance during	
		Upper Transit of Moon.	Lower Transit of Moon.
Moon's declination, North.	30°	10°	70°
	20°	20°	60°
	10°	30°	50°
Moon in Equator.	0°	40°	40°
Moon's declination, South.	10°	50°	30°
	20°	60°	20°
	30°	70°	10°

The differences of the Moon's zenith distance in her various hours of transit are so small that they have been neglected in this table entirely.

Table 23.—Zenith Distance of Moon at the Time of the Generating Lunar Transit, from Low Water on Parallel 40° North.

Lunar Declination, North.	Zenith Distance During		Lunar Declination, South.	Zenith Distance During	
	Upper Lunar Transit. (Second Lunar Group.)	Lower Lunar Transit. (First Lunar Group.)		Upper Lunar Transit. (First Lunar Group.)	Lower Lunar Transit. (Second Lunar Group.)
30°	78½°	75°	30°	75°	78½°
25°	81°	77°	25°	77°	81°
20°	83°	79°	20°	79°	83°
15°	85½°	81°	15°	81°	85½°
10°	87½°	83½°	10°	83½°	87½°
5°	90°	86°	5°	86°	90°
0°	88°	88°	0°	88°	88°

Table 24.—Zenith Distances of Sun at the Time of the Generating Lunar Transit, from High Water on Parallel 40° North, for every hour of Lunar Transit and every 10° of Solar Declination.

Hour of Moon's Transit.	Zenith Distances of Sun when he Declines NORTH.				SOUTH.		
	23½°	20°	10°	0°	10°	20°	23½°
	degrees.	degrees.	degrees.	degrees.	degrees.	degrees.	degrees.
1	17½	21	31	41	51	60½	64
2	23½	26½	35	44	53	63	66½
3	31½	34	41½	49½	58	67	70
4	41	43	50	57	65	73	76
5	53	55	61½	68	75	82½	85
6	66	68	74½	81	87	*86*	*83½*
7	79½	81½	88	*85½*	*79*	*73*	*70½*
8	*88*	*85½*	*78*	*71½*	*65*	*58½*	*50½*
9	*77½*	*75*	*67*	*59*	*52*	*45*	*43*
10	*70*	*66½*	*57½*	*49*	*41*	*33*	*30*
11	*65*	*62*	*52½*	*42½*	*33½*	*24*	*20½*
12	*64*	*60½*	*50½*	*40½*	*30½*	*20½*	*17*
13	*64*	*60½*	*51*	*41*	*31*	*21*	*17½*
14	*66½*	*63*	*53*	*44*	*35*	*26½*	*23½*
15	*70*	*67*	*58*	*49½*	*41½*	*34*	*31½*
16	*76*	*73*	*65*	*57*	*50*	*43*	*41*
17	*85*	*82½*	*75*	*68*	*61½*	*55*	*53*
18	83½	86	*87*	*81*	*74½*	*68*	*66*
19	70½	73	79	85½	*88*	*81½*	*79½*
20	56½	58½	65	71½	78	85½	88
21	43	45	52	59	67	75	77½
22	30	33	41	49	57½	66½	70
23	20½	24	33½	42½	52½	62	65
24	17	20½	30½	40½	50½	60½	64

NOTE.—The italicized figures in this table refer to lower, the non-italicized to upper transit of Sun.

(*See Table 25.*)

It may be mentioned here as a matter of general interest that the zenith distances of the Sun from high water stand in this relation to the zenith distances of the Moon from high water, that on the annual average the greater zenith distances of one heavenly body are always accompanied by the greater zenith distances of the other, and the smaller zenith distances of one are always accompanied by

the smaller zenith distances of the other in creating tides, all of which is set forth by the following table:

Table 26.—Showing the Relation between Mean Solar Zenith Distance and Mean Lunar Zenith Distance from High Water on Latitude 40° North.

To a Mean Annual Lunar Zenith Distance from High Water of—	Pertains a Mean Annual Solar Zenith Distance from High Water of—
degrees.	degrees.
15	43
25	46
35	53
45	62½
55	67½
65	70

It will be noted that the above tables all refer to latitude 40° North, the writer's original intention having been to utilize tidal observations made at Philadelphia for the practical part of his paper. This intention was abandoned after the tables had been constructed, and it was not considered necessary, for the sake of the small difference in latitude, to go through the laborious process of reconstructing them.

The equatorial positions being the mean positions of the heavenly bodies, we derive their mean zenith distances by taking the mean of the various zenith distances during such equatorial positions, from Tables 22, 23, 24 and 25, as follows:

Mean Zenith Distances of the Heavenly Bodies on Latitude 40° North, from High and Low Water at the Time of the Generating Lunar Transit.

(1.) Mean Zenith distance of Moon from High Water, 40°
(2.) Mean Zenith distance of Moon from Low Water, 83°
(3.) Mean Zenith distance of Sun from High Water, 57°
(4.) Mean Zenith distance of Sun from Low Water, 62°
} Approximately.

These mean zenith distances correspond to the mean heights of high and low water on latitude 40° North, and a period of nineteen years observed continuously on such latitude would render such means of zenith distances; if, however, as is the case here, a shorter and not continuous period only is available, then the mean sidereal conditions of such shorter and not continuous period may be different, and the means of high and low water obtained from it will

then have to be corrected accordingly in order to find "mean high water" and "mean low water."

According to Tables 22, 23, 24 and 25, the zenith distances of the heavenly bodies were entered in the first reduction for each observation; then the second reductions were made and grouped in such a way as to render apparent the amount of the various heavenly influences. The following tables show the means thus obtained for the height of low water.

The Low Water Observations Grouped so as to Render Apparent the Influence of Lunar Parallax on the Height of Low Water at Cape Henlopen.

MEANS OF SECOND REDUCTION.

Table 27.—The Higher Parallaxes.

Number of Observations	Reading of Gauge.	Moon's Parallax	Zenith Distance of Moon.	Zenith Distance of Sun.
	feet.	minutes.	degrees.	degrees.
114	—0·20	58·5	83	35
83	—0·48	58·7	83	54½
112	—0·62	58·8	84	64¾
101	—0·78	59·2	83¼	74½
120	—0·88	59·2	84¼	84½

Table 28.—The Lower Parallaxes.

Number of Observations.	Reading of Gauge.	Moon's Parallax.	Zenith Distance of Moon.	Zenith Distance of Sun.
	feet.	minutes.	degrees.	degrees.
137	+0·25	55·0	83	35
72	—0·01	54·9	82	54½
108	—0·21	55·0	83	64¾
92	—0·40	54·9	83½	74½
93	—0·46	54·9	83½	84½

The Low Water Observations Grouped so as to Render Apparent the Influence of Lunar Zenith Distance on the Height of Low Water at Cape Henlopen.

MEANS OF SECOND REDUCTION.

Table 29.—The Greater Lunar Zenith Distances.

Number of Observations.	Reading of Gauge.	Moon's Parallax.	Zenith Distance of Moon.	Zenith Distance of Sun.
	feet.	minutes.	degrees	degrees.
126	—0·06	56·5	86	32
69	—0·27	57·2	86	54¾
120	—0·45	57·0	86½	65
101	—0·65	57·1	86	74½
120	—0·66	57·6	86½	84½

Table 30.—The Smaller Lunar Zenith Distances.

Number of Observations.	Reading of Gauge.	Moon's Parallax.	Zenith Dirtance of Moon.	Zenith Dirtance of Sun.
	feet.	minutes.	degrees.	degrees.
125	+0·16	56·6	80	38
86	—0·25	56·7	79½	54½
100	—0·39	56·8	80	64½
92	—0·55	57·2	80	74½
93	—0·74	57·1	80½	84½

It will be noticed that the above tables for low water are arranged for progressive values of solar zenith distance, which is

the most decisive factor in the variations of height of low water, the lunar zenith distance from low water, and consequently the lunar contribution towards it varying within comparatively small limits only. In Tables 27 and 28, the differences in lunar zenith distance are very slight; the difference then, appearing in the height of low water for the same mean solar zenith distance in the two tables, must be attributed almost entirely to the difference in lunar parallax; in a similar way, the differences in height appearing in Tables 29 and 30 must be attributed to the differences in lunar zenith distance. The summing up of Tables 27 and 28 thus renders the result that for a difference of 19·7 minutes in lunar parallax there is a difference of 2·13 feet in height of low water, or "for a difference of 1 minute in lunar parallax, there is a difference of about 0·11 feet in height of low water."

Again, the summing up of Tables 29 and 30 renders the result that between 80° and 86° of lunar zenith distance, there is a difference of 0·05 feet; that is to say, that the point corresponding to 80° on the lunar wave is that much elevated above the one corresponding to 86°.

Table 31 renders the general means of all low water observations grouped according to solar zenith distance, and Table 32 renders these means as corrected to 57 minutes of parallax and 83° of lunar zenith distance; the figures of this table, therefore, can be utilized to construct the solar wave corresponding to mean sidereal conditions.

Table 31.—General Means of all Observations of Height of Low Water.

Number of Observations.	Reading of Gauge	Moon's Parallax.	Zenith Distance of Moon.	Zenith Distance of Sun.
	feet.	minutes.	degrees.	degrees.
251	+0·05	56·6	83	35
155	—0·26	57·0	82½	54½
220	—0·42	56·9	83½	64¾
193	—0·60	57·2	83½	74½
213	—0·70	57·3	84	84½

Table 32.—General Means as Corrected to Mean Parallax and Mean Lunar Zenith Distance.

Number of Observations.	Reading of Gauge.	Moon's Parallax.	Zenith Distance of Moon.	Zenith Distance of Sun.
	feet.	minutes.	degrees.	degrees.
251	+0·01	57·0	83	35
155	—0·26	57·0	83	54½
220	—0·43	57·0	83	64¾
193	—0·58	57·0	83	74½
213	—0·66	57·0	83	84½
Means:	—0·38	57·0	83	62⅜

The mean of Table 32 has to be reduced to the mean solar

zenith distance, in order to obtain mean low water, and from the heights for 54½° and 64¾° solar zenith distance contained in the table, we find that the required reduction amounts to + 0·01, and we therefore obtain as a mean from 1,032 observations:

Height of mean low water at Cape Henlopen — 0·37.

The sidereal conditions for the five positions of Table 32 being equal, with the exception of solar zenith distance, which is a different one for each position, it is clear that the differences in reading must be ascribed to such differences in solar zenith distance. By platting the figures of Table 32 in a suitable way, a diagram is therefore obtained of the contour of the solar component wave. This platting has been executed in *Fig. 22*, the readings of Table 32 having previously been reduced to "*mean low water equal to zero*," by simply adding 0·37 to each of them.

The following tables show the means derived from the second reduction of high water observations, and grouped so as to render apparent the amount of the various heavenly influences.

The High Water Observations Grouped so as to Render Apparent the Influence of Lunar Parallax on the Height of High Water at Cape Henlopen.

MEANS OF SECOND REDUCTION.

Table 33.—The Higher Parallaxes.

Number of Observations.	Reading of Gauge.	Moon's Parallax.	Zenith Distance of Moon.	Zenith Distance of Sun.
	feet.	minutes.	degrees.	degrees.
58	4·85	59·6	17	46
115	4·39	58·8	24⅓	49
85	4·17	58·6	34½	52
74	3·80	58·6	45	62½
104	3·54	58·8	55	68
51	3·43	59·9	63	68½

Table 34.—The Lower Parallaxes.

Number of Observations.	Reading of Gauge.	Moon's Parallax	Zenith Distance of Moon.	Zenith Distance of Sun.
	feet.	minutes.	degrees	degrees.
77	3·94	54·6	16½	50
109	3·90	55·0	24¼	53½
72	3·74	55·3	35	58½
62	3·52	55·2	45	61
106	3·31	55·0	56	67
70	2·85	54·7	63½	70

The High Water Observations Grouped so as to Render Apparent the Influence of Solar Zenith Distance on the Height of High Water at Cape Henlopen.

MEANS OF SECOND REDUCTION.

Table 35.—The Greater Solar Zenith Distances.

Number of Observations.	Reading of Gauge.	Moon's Parallax.	Zenith Distance of Moon.	Zenith Distance of Sun.
	feet.	minutes.	degrees.	degrees.
78	4·14	56·4	17	63
117	4·00	56·7	24½	68½
91	3·70	56·8	34¾	68
49	3·31	56·9	44¾	80
93	3·26	56·9	55⅔	79
52	3·01	56·3	63	78½

Table 36.—The Smaller Solar Zenith Distances.

Number of Observations.	Reading of Gauge.	Moon's Parallax.	Zenith Distance of Moon.	Zenith Distance of Sun.
	feet.	minutes.	degrees.	degrees.
57	4·59	57·2	16⅓	28
107	4·32	57·2	24	32½
66	4·35	57·5	34½	38
87	3·87	57·1	45	51½
117	3·55	56·8	55½	58
69	3·16	57·3	63⅓	62⅔

From Tables 33 and 34, we derive the following differences in height for the corresponding differences in lunar parallax:

Difference in Lunar Parallax.	Difference in Reading of Gauge.
minutes.	feet.
5·0	0·91
3·8	0·49
3·3	0·43
3·4	0·28
3·8	0·23
5·2	0·58

The solar zenith distances in the two tables do not, however, agree sufficiently closely to permit of deducing direct from these figures the influences of lunar parallax. The required correction we may take from the contour of the solar wave, *Fig. 22*, and we then obtain:

Table 37.—The Influence of Lunar Parallax on the Height of High Water at Cape Henlopen, as deduced from Tables 33 and 34:

Lunar Zenith Distance.	Difference in Parallax.	Corresponding Difference in Reading of Gauge.	Difference in Reading of Gauge per Minute of Parallax.
16¾	5·0	0·86	0·17
24¼	3·8	0·43	0·11
34¾	3·3	0·33	0·10
45	3·4	0·30	0·09
55½	3·8	0.24	0·06
63	5·2	0·56	0·11
Means:	4·1	0·45	0·11

The last column of this table, containing the differences in height per 1 minute of lunar parallax, seems to indicate that during the lesser lunar zenith distances the influence exercised by lunar parallax is more pronounced than during the greater lunar zenith distances, an indication which is in perfect accordance with theory. The results in this direction, however, are not sufficiently well defined to make any distinction, and we shall, therefore, assume the general mean rendered by Table 37, of 0·11 per minute of parallax for all positions of the Moon. This value is, as will be remembered, precisely the same that was found for the influence of lunar parallax on the height of low water.

The differences in height of high water caused by solar zenith distances, as set forth in Tables 35 and 36, afford a check on the correctness of the solar wave as constructed from the low water observations. If the heights in both tables are corrected to an equal parallax, then we derive

Solar Zenith Distance. Table 35.	Solar Zenith Distance. Table 36.	Difference in Reading of Gauge, due to Difference in Adjoining Solar Zenith Distances.
degrees.	degrees.	
28	63	0·36
32½	68½	0·27
38	68	0·57
51½	80	0·54
58	79	0·30
62⅔	78½	0·05

A comparison of these differences with the differences in height between the points in question of the solar wave (*Fig. 22*) will show that on the general average both agree tolerably well.

The following tables render the general means of the height of high water:

Table 38.—General Means of all Observations of Height of High Water.

Number of Observations.	Reading of Gauge.	Moon's Parallax.	Zenith Distance	
			of Moon.	of Sun.
	feet.	minutes.	degrees.	degrees.
135	4·33	56·7	16⅔	48½
224	4·15	57·0	24⅓	51
157	3·97	57·1	34¾	55
136	3·67	57·0	45	61⅔
210	3·42	56·8	55½	67½
121	3·09	56·9	63	69½

Table 39.—General Means as Corrected to Mean Lunar Parallax and Mean Solar Zenith distance.

Number of Observation.	Reading of Gauge.	Moon's Parallax.	Zenith Distance	
			of Moon.	of Sun.
	feet.	minutes.	degrees.	degrees.
135	4·23	57·0	16⅔	57
224	4·06	57·0	24⅓	57
157	3·93	57·0	34¾	57
136	3·75	57·0	45	57
210	3·61	57·0	55½	57
121	3·31	57·0	63	57
Means:	3·81	57·0	39⅞	57

We obtain from Table 39 as a mean of 983 observations,

Height of mean high water at Cape Henlopen = 3·81

It will be noticed that the above tables for high water are arranged for progressive values of lunar zenith distance. In Table 39, the sidereal conditions are equal throughout with the exception of lunar zenith distance, the differences in height (or reading,) therefore, appearing in this table, must be due to the differences in lunar zenith distance, and we are thus enabled from the general means rendered by this table to construct the contour of the lunar wave. This construction has been made in *Fig 23*, the readings of Table 39 having been previously reduced to "*mean low water equal to zero*," by simply adding 0·37 to each of them.

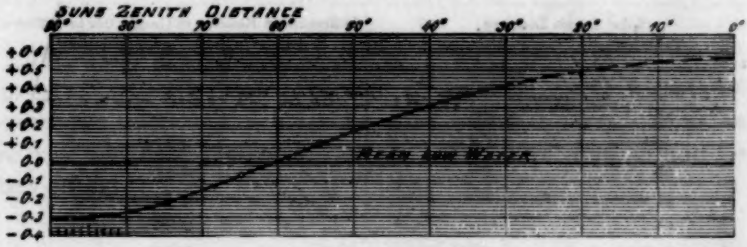


Fig. 22.—The Solar Wave as Constructed from the Corrected General Means of 1,032 Low Water Observations at Cape Henlopen

Lunar parallax . . . . . = 57′
Moon's Zenith distance . . . . . = 83°

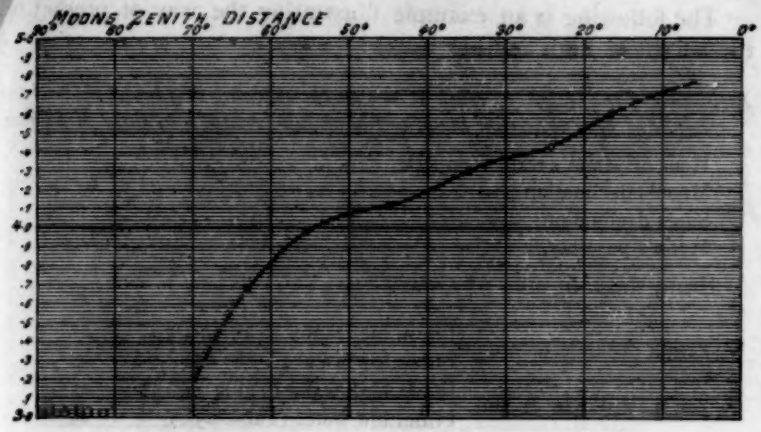


Fig. 23.—The Lunar Wave as Constructed from the Corrected General Means of 983 High Water Observations at Cape Henlopen

Lunar parallax . . . . . = 57′
Sun's zenith distance . . . . . = 57°

While the contour of the solar wave is a regular curve, that of the lunar wave is very irregular, a result which the writer is not in a position to explain beyond this, that a greater number of observations than were at his disposal would probably yield a more regular curve.

Before giving an example, showing the way in which the curves of *Fig. 22* and *Fig. 23* are utilized in order to make predictions, the corrections which, as found before, have to be applied outside of them are herewith recapitulated as follows:

Corrections for Height of Low Water.

For 1 minute additional lunar parallax, there is to be subtracted 0·11 of the height.

For 1° additional of lunar zenith distance, there is to be subtracted 0·01 of the height.

Correction for Height of High Water.

For 1 minute additional lunar parallax, there is to be added 0·11 to the height.

The height of mean high water above mean low water, in other words, the mean rise and fall, has been found as follows:

Mean rise and fall at Cape Henlopen, ... 4·18

The following is an example illustrating the way of predicting the height of high and low water:

Prediction of the Height of High Water on the Evening of June 26th, and the Height of Low Water on the Morning of June 27, 1884, at Cape Henlopen.

Hour of Moon's transit (upper),	2nd
Moon's declination,	19° N.
Sun's declination,	21° N.
Moon's parallax,	61′

From these figures we derive:

Zenith distance of Moon	from high water (Table 22),	21°
	from low water (Table 23),	83°
Zenith distance of Sun	from high water (Table 24),	25½°
	from low water (Table 25),	85°

In taking these zenith distances from the tables, it has to be remembered that the high and the low water, which we are about to predict, are incident on an upper lunar transit during Northern lunar declination, and that therefore they belong to the second lunar group.

If now we take from our diagrams, *Fig. 22* and *Fig. 23*, the solar and lunar elevations, corresponding to the above found zenith distances, introducing also the other corrections, then we obtain:

Height of High Water.

(1.) Lunar contribution,	4·50
(2.) Solar contribution, being the difference between the elevation at 57° and at 25½° of the solar wave,	+ 0·40
(3.) Correction for 61′ lunar parallax,	+ 0·44
Predicted height,	5·34

Height of Low Water.

(1.) Solar contribution,	— 0·30
(2.) Correction for lunar zenith distance,	
(3.) Correction for 61′ lunar parallax,	— 0·44
Predicted height,	— 0·74

The actually observed heights were:

High water,	5·54
Low water,	— 1·01

all of which figures refer to the plane of mean low water.

Table 25.—Zenith Distances of Sun at the Time of the generating Lunar Transit, from Low Water on Parallel 40° North, for every Hour of Lunar Transit, every 10 Degrees of Solar Declination and every 30 Degrees of Lunar Declination.

Hour of Moon's Transit.	Zenith Distances of Sun when he Declines — NORTH. 23½ DEGREES. The Moon Declines			NORTH. 20 DEGREES. The Moon Declines			NORTH. 10 DEGREES. The Moon Declines			0 DEGREES. The Moon Declines			SOUTH. 10 DEGREES. The Moon Declines			SOUTH. 20 DEGREES. The Moon Declines			SOUTH. 23½ DEGREES. The Moon Declines		
	30° in 1 Group.	0° Equator.	30° in 2 Group.	30° in 1 Group.	0° Equator.	30° in 2 Group.	30° in 1 Group.	0° Equator.	30° in 2 Group.	30° in 1 Group.	0° Equator.	30° in 2 Group.	30° in 1 Group.	0° Equator.	30° in 2 Group.	30° in 1 Group.	0° Equator.	30° in 2 Group.	30° in 1 Group.	0° Equator.	30° in 2 Group.
	deg.	deg.	deg.	deg.	deg.	deg.	deg.	deg.	deg.	deg.	deg.	deg.	deg.	deg.	deg.	deg.	deg.	deg.	deg.	deg.	deg.
1	78	83	88	80	85½	89½	80½	88	82	87	81½	75½	80½	75	69	74½	69	63	72	67	61
2	86	89	84½	88	87	82	85	80	74½	78½	73	67½	72	66½	60½	66	60½	54½	63½	58½	52½
3	86½	82	78	84	79½	75	76½	72	67	69½	64½	59½	62½	58	52½	56½	51½	46	54½	49½	44
4	78	74½	71½	75	71½	68½	67	63½	59½	59½	55½	51	52	48	43	46	41	36	43½	39	33½
5	70½	68½	66	67	65	62½	59	56	53	50	47	43½	42½	38	35	35	30½	26	32	28	24
6	63½	64	63½	62	60½	60	52½	51	50	43	41	40	33½	31½	30	24½	21½	20	21½	18	16½
7	63½	64	65½	60	60½	62	50	51	52½	40	41	43	30	31½	33½	20	21½	24½	10½	18	21½
8	66	68½	70½	62½	65	67	53	56	59	43½	47	50	35	38½	42½	26	30½	35	24	28	32
9	73	76	80½	69½	73	77½	61	65	69½	52	57	62½	45	50	55	38	43	49	36	41	47
10	80½	85½	89½	77½	83	88	70	75½	81	62½	68½	74	56	62	67½	49½	55½	61½	47½	53½	59
11	89½	84½	79	88	87	80½	81½	80½	88	74½	80	85½	68	73½	79	62	67½	73	60	65½	71
12	80½	75	70	82½	77½	72	89	84	78	84	**90**	84½	78	83½	89	71½	77	82½	69½	75	80
13	72	67	61	74½	69	63	80½	75	69	87	81½	75½	80½	88	82	80	85½	89½	78	83	88
14	63½	58½	52½	66	60½	54½	72	66½	60½	78½	73	67½	85	80	74½	88	87	82	80	89	84½
15	54½	49½	44	56½	51½	46	62½	58	52½	69½	64½	59½	76½	72	67	84	79½	73	86½	82	78
16	43½	39	33½	46	41	36	52	48	43	59½	55½	51	67	63½	59½	73	71½	68½	78	74½	71½
17	32	28	24	35	30½	26	42½	38	35	50	47	43½	59	56	53	67	65	62½	71½	68½	66
18	21½	18	16½	24½	21½	20	33½	31½	30	43	41	40	52½	51	50	62	60½	60	65½	64	63½
19	16½	18	21½	20	21½	24½	30	31½	33½	40	41	43	50	51	52½	60	60½	62	63½	64	65½
20	24	28	32	26	30½	35	35	38½	42½	43½	47	50	53	56	59	62½	65	67	66	68½	70½
21	36	41	47	38	43	49	45	50	55	52	57	62½	61	65	69½	69½	73	77½	73	76	80½
22	47½	53½	59	49½	55½	61½	56	62	67½	62½	68½	74	70	75½	81	77½	83	88	80½	85½	89½
23	60	65½	71	62	67½	73	68	73½	79	74½	80	85½	81½	86½	88	88	87	80½	89½	84½	79
24	69½	75	80	71½	77	82½	78	83½	89	84	**90**	84½	89	84	78	82½	77½	72	80½	75	70

NOTE.—The italicized figures in this table refer to lower, the non-italicized to upper transit of Sun.

(Gieseler.)

r. Frank. Inst., Vol. CXX. Nov., 1885.

The limited number of observations available have not allowed, as mentioned before, to determine the influence of solar distance, neither has it been possible to determine the influence of winds, which, as far as prediction is concerned, might possibly be utilized on the basis of the prevailing direction and average force for certain periods of the year. The writer, therefore, desires to say in conclusion that his object in publishing this paper, mainly was to call attention to a system of prediction, the application of which, on a broader basis than was at his disposal, will in his opinion prove fruitful. Conscious of the above mentioned and other shortcomings, he does not claim for the practical example given any more than results of ordinary value, obtainable doubtless also by other methods with the same degree of exactitude.

CRUSHING LIMIT OF COLUMNS.—In preparing a plan for an electric light-house M. Bourdais, the architect of the Palace of the Trocadero, investigated the height to which a column of different materials could be raised without crushing under its own weight. The weight of a pyramid with a square base may be expressed by the equation:

$$P = D^2 \frac{h}{3} \delta$$

in which D represents the side of the base of the pyramid, h the height, and δ the density.

The resistance is: $R = \frac{P}{D^2}$

Hence $R = \frac{1}{3} h \delta$

$$h = \frac{3R}{\delta}.$$

If we take for the limiting value of R one-sixth of the load, which produces crushing in iron, and one-twentieth for different varieties of stone, we may deduce the following table:

MATERIAL.	R.	δ.	H.
Porphyry,	2,470,000	2,870	2,550 metres.
Iron,	6,000,000	7,800	2,280 "
Granite,	800,000	2,780	900 "
Bagneux freestone,	440,000	2,400	540 "
Saint nom stone,	230,000	2,300	300 "
Banc royal,	60,000	1,700	100 "
Vergelé,	30,000	1,500	60 "

Such are the practical limits to which a pyramid might be raised in the respective materials. It is evident that the Egyptians, in the great pyramid of Cheops, stopped far below the limit. If the prismatic form were adopted, the height could be only one-third as great.—*Lumière Électrique*, February 21, 1885.

CYLINDER CONDENSATION IN STEAM ENGINES. AN EXPERIMENTAL INVESTIGATION.

BY CHAS. L. GATELY, M. E., AND ALVIN P. KLETZSCH, M. E.

[*Presented to the American Association for Advancement of Science; Ann Arbor Meeting, 1885, with an Introduction by* PROFESSOR R. H. THURSTON.]

[*Continued from October Issue, page 298.*]

9. THE METER.

The water meter used to indicate the amount of feed water that was pumped into the boiler was carefully standardized by allowing exactly 20 cubic feet of water to flow through.

It was necessary to use a small barrel, owing to the position in which the meter was placed. It will be seen from the data taken at the time, that in 20 cubic feet the difference in the actual and calculated number of pounds flowed through is only three pounds, this difference being no doubt due to error in observing the readings, rather than to any fault of the meter itself. The following figures are the weights of the water and barrel, and of the barrel itself:

First barrel full,	232·	
First barrel empty,		37·
Second barrel full,	176·	
Second barrel empty,		37·5
Third barrel full,	174·	
Third barrel empty,		37·5
Fourth barrel full,	177·5	
Fourth barrel empty,		37·5
Fifth barrel full,	155·5	
Fifth barrel empty,		37·5
Sixth barrel full,	188·5	
Sixth barrel empty,		37·5
Seventh barrel full,	188·5	
Seventh barrel empty,		38·
Eighth barrel full,	163·	
Eighth barrel empty,		38·
Ninth barrel full,	128·5	
Ninth barrel empty,		38·
Total,	1583·5	338·5
Difference,	1245. pounds.	

The thermometers used were new and made by J. W. Queen & Co., Philadelphia, and as by comparison with each other they were found alike, further standardizing was not deemed necessary.

10. THE WEIR.

The tumbling bay was very carefully tested, in order to ascertain the coefficient of contraction, or the ratio which the cross section of the most contracted part of the steam flowing from the notch, bears to the area of the notch at a given head.

The test was made as follows: Water was allowed to flow through the condenser into the weir; after the surface became calm the height of water over the notch was read, the time noted and water allowed to flow over for two minutes, all of which was caught and weighed. This was repeated four times, and the data taken; together with the method used in calculating the average coefficient of contraction is given below:

TEST NO. 1.

Weight of barrel No. 1, full,	267·5	
" " " 1, empty,		40·5
" " " 2, full,	243·5	
" " " 2, empty,		36·5
" " " 3, full,	146·	
" " " 3, empty,		36·5
Total,	657·	113·5
Difference,	543·5 pounds.	

Breadth of notch $= b = 6$ inches $= 0{\cdot}5$ foot.
Height over notch board $= h = {\cdot}125$ foot.
Time of flow $= 2$ minutes.
Flow in cubic feet, per second,

$$= \frac{543{\cdot}5}{62{\cdot}4 \times 120} = {\cdot}07258 = Q.$$

Substituting these values in Rankine's Formula, page 93, of "The Steam Engine," which is

$$Q = \tfrac{2}{3}\, c\, b\, h \sqrt{2\, g\, h}$$

Where Q, b and h, are, as stated before, $2\ g$ being 64·4 and $\sqrt{2\, g\, h}$, the velocity due to the height h and c the coefficient of contraction to be found, hence

$$\cdot 07258\, b\, h = \cdot 5 \times \cdot 125 = \cdot 0625$$

$$\sqrt{2\, g\, h} = \sqrt{64 \cdot 4} \times \cdot 125 = 2 \cdot 837$$

$$b\, h \sqrt{2\, g\, h} = \cdot 0625 \times 2 \cdot 837 = \cdot 1773$$

$$c = \tfrac{3}{2}\, Q \frac{1}{b\, h \sqrt{2\, g\, h}} = \tfrac{3}{2} \times \cdot 07258 \times \frac{1}{\cdot 1773}$$

$$c = \cdot 614.$$

TEST NO. 2.

Weight of barrel No. 1, full,	252·5	
" " " 1, empty,		40·5
" " " 2, full,	188·	
" " " 2, empty,		36·5
" " " 3, full,	113·5	
" " " 3, empty,		26·5
" " " 4, full,	137·	
" " " 4, empty,		26·5
Total,	691·	130·
Net flow,	561· pounds.	

Here $b = 6\cdot$ inches.

$h = 1\cdot 5$ inches.

Time of flow = 2 minutes.

and substituting in formula as in previous test

$$c = \cdot 633$$

TEST NO. 3.

Weight of barrel No. 1, full,	445·	
" " " 1, empty,		61·
" " " 2, full,	151·	
" " " 2, empty,		37·
Total,	596·	98·
Net flow,	498· pounds.	

Here $b = 6\cdot$ inches.

$h = 1\cdot 416$ inches.

And time of flow = 2 minutes.

Hence

$$c = \cdot 6133$$

TEST NO. 4.

Weight of barrel No. 1, full,	412·5	
" " " 1, empty,		61·5
" " " 2, full,	201·5	
" " " 2, empty,		38·
Total,	614·	99·5
Net flow,	514·5 pounds.	

Here $b = 6{\cdot}0$ inches.
$h = 1{\cdot}416$ inches.
And time of flow = 2 minutes.

Hence

$$c = {\cdot}6233$$

Average coefficient of contraction of tests No. 1, No. 2, No. 3 and No. 4, would give

$$c = {\cdot}6233$$

This value for c being used to calculate the flow from the condenser in the trials.

A Fairbank's standard platform scale was used in weighing the power developed by the brake and, by careful comparison with others, was found to be correct.

The weight registered by the scales, owing to a part of the weight of the brake falling on it, was found by weighing the upright standards by themselves, and then the beams and tie-rods, which formed the levers of the brake, the design of which was such, that all the weight of the standards and one-half the weight of the beams, etc., would directly tend to increase the reading, hence this had to be ascertained in order to get the true scale reading. It amounted to 202·5 pounds, which is to be subtracted from all the scale readings in the logs of the trials.

Weight of uprights, etc., ...	131·5 pounds.
One-half weight of beams, tie-rods, etc., ...	71· "
Total, ...	202·5 "

A Hawkin's speed counter was used to record the total number of revolutions made by the engine during each trial, receiving its motion from the eccentric rod.

As before stated, leakage of piston was looked for, and its non-existence determined first by disconnecting the valve gear, stopping the piston about midway of the stroke, and introducing water through one of the steam ports, at the same time closing the exhaust at that, and opening it at the other end, so that any water getting by the piston would run out through that port; but upon repeated trials it was found to be water-tight.

Next, steam was admitted at one end with all the other valves closed, and the indicator cock at the other end was opened, but no steam issued from it.

The clearance was not determined by us, but obtained from Mr. Wm. A. Harris, the builder; he gave the following figures for all his engines of this size, 297·4 cubic inches, for each end.

II. METHOD OF OPERATION OF ENGINE, BOILERS AND APPARATUS.

In running the trials, in order to preserve constant conditions of pressure, speed and ratio of expansion, the greatest care and watchfulness was required. The fires in the furnaces were thoroughly cleaned at night. Fires were restarted at 6 o'clock in the morning, so that by 7·30 they were burning freely. Before beginning a trial, the pressure was allowed to run up a few pounds higher than that which was to be run, the regulator was then blocked up to a point which would give the required cut-off, as determined by previous observations, and the different points indicated by marks upon the upright rod of the regulator. The throttle was then opened, and the pressure of the brake applied, until the speed was as required.

The fireman and his assistant were then cautioned to keep the pressure constant. We were fortunate in securing the services of the fireman who had had the care of the boilers and engine since they were put down. The only condition remaining was the speed, which was entirely controlled by the brake.

When the proper speed was reached, and all was ready for the start, the height of water in the boiler gauges noticed, meter reading taken, the speed counter read and the time recorded from which instant the trial began, the longest run being three hours, and the shortest one-and-a-half hours. Indicator cards were taken simultaneously from each end of the cylinder as soon after the trial began as it was possible, and continued throughout the run at intervals of every fifteen minutes in the longer trials, and every ten minutes in the shorter. Besides these, the following observations were taken at the same time, as shown by the revised logs of trials which are given:

Temperature of feed water.
Temperature of injection water.
Temperature of weir.
Pressure on brake scales.
Pressure of boiler.
Vacuum in condenser.

Head of water over notch in weir.

These readings were also taken throughout the trials at regular intervals, as shown by the logs.

12. SPECIAL POINTS.

Calorimeter tests were made during each trial, to determine the percentage of priming, and the records will be found annexed to the logs of trial.

The water used to keep the brake pulley cool entered on one side of the drum or pulley, and was discharged on the other side, by means of a scoop, into the hot well from which it was pumped into the boilers, often entering the hot well at a temperature over 100° Fahrenheit, a rise in some cases of 40°. In the every-day work of the engine, the condensing water is used for feed, but in our trials the pipe leading from the discharge pipe to the hot well was closed, so that all the water would pass through the weir as previously detailed.

In closing the trials, particular care was taken that the height of the water in the boiler gauges should exactly correspond with the height at the beginning, so that the meter readings would show the amount of water evaporated. The final meter reading was then taken and the trial concluded.

The readings of the micrometer screw and hook gauge were repeated twice at each observation, to avoid errors entering from this cause, and not even in one instance did the height of the water over the notch vary by 0·001 of an inch.

Although we make no use of the brake readings in our calculations, they were taken in order to complete the data, for any future use to which it may be put. In test Nos. 3 and 4, Case I, which were made on Sunday, May 25th, it was found necessary to use three boilers. When making our preliminary examination, it was understood that we could not have the use of this third boiler, it being used during the week to supply steam to the mill, therefore the water meter was placed in the feed pipe between this and the two boilers that were regularly used, so that the water pumped into the third boiler would not show on the dial, hence in the logs of this trial the meter readings do not appear.

Deductions have to be made from the water as shown by the meter, to the extent of that which was allowed to flow through the

drip-pipe as previously mentioned. In order to ascertain the temperature of the feed water, together with that which was condensed and allowed to flow in the calorimeter trials, these amounts are entered in the last right hand column of each log.

CHAPTER III.

13. ACCOUNT OF THE EXPERIMENTAL INVESTIGATION IN DETAIL.

From the limited investigations of others, and from thermodynamics, it is clearly shown that variations of ratio of expansion, variation of piston speed or time of exposure, and variation of change of pressure or temperature, all exert some influence on the amount of cylinder condensation which takes place in the steam engine. The exact part which each of these causes play in an engine running under ordinary conditions of daily work has never been determined experimentally. With three variables entering the problem at the same time, it would be almost, if not absolutely, impossible to do it. Hence it is we divided our experiments into three parts, making four separate sets of tests, with the view of determining the effect of each of the above factors, and the variation caused by change in any of them. Starting with the effect on cylinder condensation, caused by the variation of the ratio of expansion or the point of cut-off, and running the condenser, we close a boiler pressure reasonably within the capacity for the two boilers to maintain during the heaviest one of the trials of the set, also selecting the speed at which, by trial, the engine was found to run best.

14. Speed and pressure were maintained as constant as possible throughout this whole set of four tests, as will be seen by referring to the logs of the trials given below; the ratio of expansion alone being allowed to vary, and that only during separate tests of each set. As before described, it was possible to secure this latter condition completely by blocking the regulator so that its action was prevented.

On May 24, 1884, at 10.16 A. M., with the boiler pressure at 62·5 pounds, the engine making 68 revolutions per minute, and cutting off at ·131 of the stroke, we commenced the first trial, stopping at 12.16 P. M., after a run of two hours.

The indicator diagrams taken, ten in number from each end, showed a constant cut-off, and, in fact, it could not be otherwise.

The greatest range in the boiler pressure was four and one-half pounds, with an average of 61·1 pounds per gauge, and with an average of 68·95 revolutions. The average revolutions and pressure carried, and the point at which the engine cut-off during the remaining three of this set of tests, are as follows:

No. of Test.	Cut Off.	Pressure.	Revolutions.
2	·330	60·1	67·32
3	·443	61·28	67·45
4	·589	56·83	68·26

giving 4·45 pounds as the greatest average variation in pressure, owing to insufficient boiler power in the fourth test, though a third one was used, when following beyond one-half stroke, and 1·63 revolutions as the greatest average variation in the speed of the engine per minute. This variation is so slight that its probable effect on the total condensation need hardly be calculated.

15. Below will be found the logs containing the data taken during each trial, together with the calorimeter trials made in connection with them:

CASE I.—CONDENSING.—VARIABLE CUT-OFF.

Table No. 1.
Test No. 1.
Date of Test, May 24th, 10·16 A. M. to 12·16 P. M.
Duration, 2 Hours.

Conditions: Constant Pressure. Constant Speed. Variable Cut-off.

	Time.	Speed Counter.	Water Meter.	Height over Notch Board.	Temperature Feed Water.	Temperature Injection Water.	Temperature Weir.	Boiler Pressure.	Vacuum Gauge.	Remarks.
Start,	10·16	6875	5093·2	2·325		66	118	62·5	22·5	
	10·31	7909		2·322		67	117	65·	22·5	
	10 36	8255								
	10·45	8875	5101·7	2·184	112	68	116	64·	22·5	$23\frac{10}{16}$ lbs. out.
	11·00	9905		2·214		67	117	62·	22 5	
	11·15	10935		2·220	115	67	118	58·	22 5	18¼ lbs. ded'ct
	11·30	11964		2·214		67	115	60·	22·5	
	11·45	12999	5124·	2·200	116	67	118	61·	22·	6½ lbs. ded'ct
	12 00	14035	5134·	2·200		67	118	59·5	22·5	
Finish,	12·16	15150	5145·2	2·625		68	110	58·	23·5	

Case I.—Condensing.—Variable Cut-off.

Table No. I.
Test No. 2.
Date of Test, May 24th, 2·36 P. M. to 4·31 P. M.
Duration, 1$\frac{55}{60}$ Hours.

Conditions: { Constant Pressure. Constant Speed. Variable Cut-off.

	Time.	Speed Counter.	Water Meter.	Height over Notch Board.	Temperature Feed Water.	Temperature Injection Water.	Temperature Weir.	Boiler Pressure.	Vacuum Gauge.	Remarks.
Start,	2·36	15593	5164·	3·252	144	71	138	62·	20·5	
	2·46	16274		3·146		70	137	58·	21·5	
	3·01	17272		3·178		71	133	60·	21·5	
	3·16	18281		3·581		72	124	59·	22·	
	3·30	19896		2·913		72	141	62·	20·	
	3·46	20304		3·132	136	72	138	62·	20·5	Deduct 25 lbs.
	3·53	20780	5214·	3·132		72	137	59·	21·	
	4·04	21528		3·127		72	138	59·	20·5	
	4·16	22344		3·319		72	133	59·	21·	
Finish,	4·31	20334	5276·	3·170		72	137	61·		

Case I.—Condensing.—Variable Cut-off.

Table No. 1.
Test No. 3.
Date of Test, May 25th, from 11·32 to 1·02 P. M.
Duration, 1½ Hours.

Conditions: { Constant Pressure Constant Speed. Variable Cut-off.

	Time.	Speed Counter.	Water Meter.	Height Over Notch Board.	Temperature Feed Water.	Temperature Injection Water.	Temperature Weir.	Boiler Pressure.	Vacuum Gauge.	Remarks.
Start,	11·32	28156	Meter not read, as all three Boilers were used.	4·208	Temperature not taken.	69	140	50·	20·	
	11·47	29129		4·564		69	147	62·	21·	
	12·02	30233		4·400		69	124	60·	22·	
	12·17	31284		4·560		69	125	55·	21·	
	12·32	32237		4·444		69	130	56·	22·	
	12·47	33262		4·564			129	58·	22·	
Finish,	1·02	34300		4·578		69	128		22·	

Case 1.—Condensing.—Variable Cut-off.

Table No. 1.
Test No. 4.
Date of Test, May 25, 1884, 2·04 P. M. to 4·04 P. M.
Duration of Test, 2 hours.

Conditions: { Constant Pressure. Constant Speed. Variable Cut off.

	Time.	Speed Counter.	Water Meter.	Height Over Notch Board.	Temperature Feed Water.	Temperature Injection Water.	Temperature Weir.	Boiler Pressure.	Vacuum Gauge.	Remarks.
Start,	2·04	34815		4·072	114	68	144	61·5	22·	Deduct 24½ lbs.
	2·19	35845		4·016		68	134	61·	22·	
	2·34	36842		4·108		67	126	62·	22·5	
	2·49	37850		4·009		68	132	62·	22·	
	3·04	38878		3·996		68	128	60·	22·	
	3·19	39910		3·996		68	128	62·	22·	
	3·34	40891		3·952		68	129	60·	22·	
	3·49	41910		3·964		68	130	62·	22·	
Finish,	4·04	42910		4·032		67	132			

Calorimeter Tests.

Case I.—Condensing.—Variable Cut-off.

In Connection with Engine Trial.	Initial Weight.	Initial Weight plus Condensing Water.	Final Weight.	Initial Temperature.	Final Temperature.	Flow in Air.	Flow in Steam.	Boiler Pressure.
				degrees.	degrees.	minutes.	minutes.	
No. 1.	68⅜	138⅜	143¾	70	144·5	1	2½	60·5
No. 2.	68⅝	138⅝	144	74	150·	1	2½	65·
No. 3.	68⅛	138⅛	142⅛	84	140·	1¼	2	57·
No. 4.	69⅝	139⅝	148¼	70	132·5	1	2	62·

16. Case II.

The next set of five tests with condenser was made with ratio of expansion and piston speed of engine constant, varying only the pressure of the boiler in the tests of this set.

Starting with an average of eighty pounds in the first test, and concluding with a pressure of 22·3 pounds in the fifth.

Table 2, lines 8 and 11, will show that the conditions of constancy of speed and ratio of expansion were maintained with a sufficient degree of accuracy, so that there can be no doubt but that any difference in the amount of condensation found to have occurred must be due to variation in the initial pressure.

The logs and trials of this set are given below:

Case II.—Condensing—Variable Boiler Pressure.

Table No. 2.
Test No. 5.
Date of Test, May 26, 1884, 8·01 to 10·1 A. M.
Duration, 2 hours.

Conditions: { Variable Pressure. Constant Speed. Constant Cut-off.

	Time.	Speed Counter.	Water Meter.	Height over Notch Board.	Temperature Feed Water.	Temperature Injection Water.	Temperature Weir.	Boiler Pressure.	Vacuum Gauge.	Remarks.
Start,	8·01	43625	5848·7	3 740		66	101	75·	22 5	
	8·22	44695	5863·	4·020	61	66	110	82·	22·	Deduct 19½ lbs.
	8·37	45765		4 020		66	108	82·	22·5	
	8·52	46795	5883·	4·020	103	66	110	80·	22·5	
	9·01	47855		4·084		66	106	80·	22·5	Boiler foaming some, 9·15 A.M.
	9·22	48845		4·028	97	66	103	75·	22·5	Deduct 18¼ lbs.
	9·37	49887	5925·	3·640		66	110	83·	22·5	
	9·52	50945	5952·5	3·650		66	104	81·	22·5	
Finish,	10.01	51907	5960.	3 620		66	111	82·	22·5	

Case II.—Condensing.—Variable Boiler Pressure.

Table No. 2.
Test No. 6.
Date of Test, May 26, 1884, 10·38 A. M. to 12·23 P. M.
Duration, 1¾ hours.

Conditions: Variable Pressure. Constant Speed. Constant Cut-off.

	Time.	Speed Counter.	Water Meter.	Height over Notch Board.	Temperature Feed Water.	Temperature Injection Water.	Temperature Weir.	Boiler Pressure.	Vacuum Gauge.	Remarks.
Start,	10·38	52610	5978·	4·120		66	111	65·	22·	
	10·53	53695		3·390	102	66	116	65·	22·25	
	11·08	54765	6000·	3·348	102	66	117	67·	22·	Deduct 12 lbs.
	11·23	55840		3·304		66	115	67·	23·	
	11·38	56865	6040·	3·375		66	108	69·	22·5	
	11·53	57895	6045	3·392		66	130	66·	22·	
	12·08	58975	6057·5	3·324		66	118	69·	22·	
Finish,	12·23	60036	6060·	3·360		66	136	67·	22·25	

Case II.—Condensing.—Variable Boiler Pressure.

Table No. 2.
Test No. 7.
Date of Test, May 26, 1884, 2·54 to 4·54 P. M.
Duration, 2 hours.

Conditions: Variable Pressure. Constant Speed. Constant Cut-off.

	Time.	Speed Counter.	Water Meter.	Height over Notch Board.	Temperature Feed Water.	Temperature Injection Water.	Temperatur: Weir.	Boiler Pressure.	Vacuum Gauge.	Remarks.
Start,	2·54	60090	6109·	3·500		68	101	52·	22·5	
	3·09	61100		3·632	118	68	104	52·	22·5	
	3·24	62125		3·516		68	108	54	22·5	
	3·39	63·30	6127·	3·432		68	110	52·	22·5	Deduct 20 lbs.
	3·54	64305		3·424		68	109	52·	22·	
	4·09	65405		3·280		68	112	52·	22·5	
	4·24	66510	6152·	3·320	110	68	118	52·	22·5	Deduct 9 lbs.
	4·39	67585	6169·	3·320		68	120	52·	22·5	
Finish,	4·54	68698	6179·	3·315		68	122	53·	22·5	

CASE II.—CONDENSING.—VARIABLE BOILER PRESSURE.

Table No. 2.
Test No. 8.
Date of Test, May 28, 1884, 7·36 to 9·36 A. M.
Duration, 2 Hours.

Conditions: { Variable Pressure. Constant Speed. Constant Cut-off.

	Time.	Speed Counter.	Water Meter.	Height Over Notch Board.	Temperature Feed Water.	Temperature Injection Water.	Temperature Weir	Boiler Pressure.	Vacuum Gauge.	Remarks.
Start,	7·36	69053	6515·	3·300		65	102	37·	22·	
	7·51	70157	6526	3·384		65	95	39·	22·5	
	8·06	71192		3·020		65	99	37·	22·5	
	8·21	72216	6540·	2·988		65	99	36·	22·5	
	8·36	73232		2·948		65	100	37·	22·75	
	8·51	74250	6545·	3·012	128	65	99	37·	23·	Deduct 20⅝ lbs.
	9·06	75274		3·010		64	102	36·	23·	
	9·21	76299	6565·	2·900	96	64	102	38·	23·	
Finish,	9·36	77323	6569·	2·904		64	101	36·	23·	Deduct 7 lbs.

CASE II.—CONDENSING.—VARIABLE BOILER PRESSURE.

Table No. 2.
Test No. 9.
Date of Test, May 27, 1884, 10·15 A. M. to 12·27 P. M.
Duration, 2 Hours.

Conditions: { Variable Pressure. Constant Speed. Constant Cut-off.

	Time.	Speed Counter.	Water Meter.	Height over Notch Board.	Temperature Feed Water.	Temperature Injection Water.	Tem,erature Weir.	Boiler Pressure.	Vacuum Gauge.	Remarks.
Start,	10·15	78302	6608·7	3·200	94	64·	88	23·	23·	Deduct 22 lbs.
	10·30	79321	6614·	2·380		64·	102	22·	23·	
	10·45	80342		2·324		64·	103	22·	23·	
	11·00	81363		2·348		64·	103	22·5	23·	
	11·15	82379		2·404		64·	102	21·	23·	
	11·30	83391	6634·	2·400		64·5	110	22·	23·	
	11·36	Stopped	on acco	unt of ec	centri	c stra	p. St	arted	again at	11·42 A. M.
	11·42	83871	6642·	2·400	118	64·5	101	26·	23·	Deduct 15¾ lbs
	11·57	84892		2·390		64 5	102	22 5	23·	
	12·12	85912	6650·	2·400		64·5	101	20·	23·	
Finish,	12·27	86935	6652·5	2·396	106	64·5	104	22·	23·	Deduct 22¾ lbs.

CALORIMETER TESTS.

Case II.—Condensing.—Variable Boiler Pressure.

In Connection with Engine Trial.	Initial Weight.	Initial Weight plus Condensing Water	Final Weight.	Initial Temperature.	Final Temperature.	Flow in Air.	Flow in Calorimeter.	Boiler Pressure.
				degrees.	degrees.	minutes.	minutes.	
No. 5.	69½	139½	145¼	67·5	152	1	2½	82·
No. 6.	70	140	146¼	69·	156	1·	3	67·5
No. 7.	70 9/16	140 9/16	145 13/16	70·	138	1	2	54·
No. 8.	70⅜	140⅜	146⅜	67·5	139	1	4	36·5
No. 9.	70⅜	140⅜	146¼	67·	140	1	6	22·5

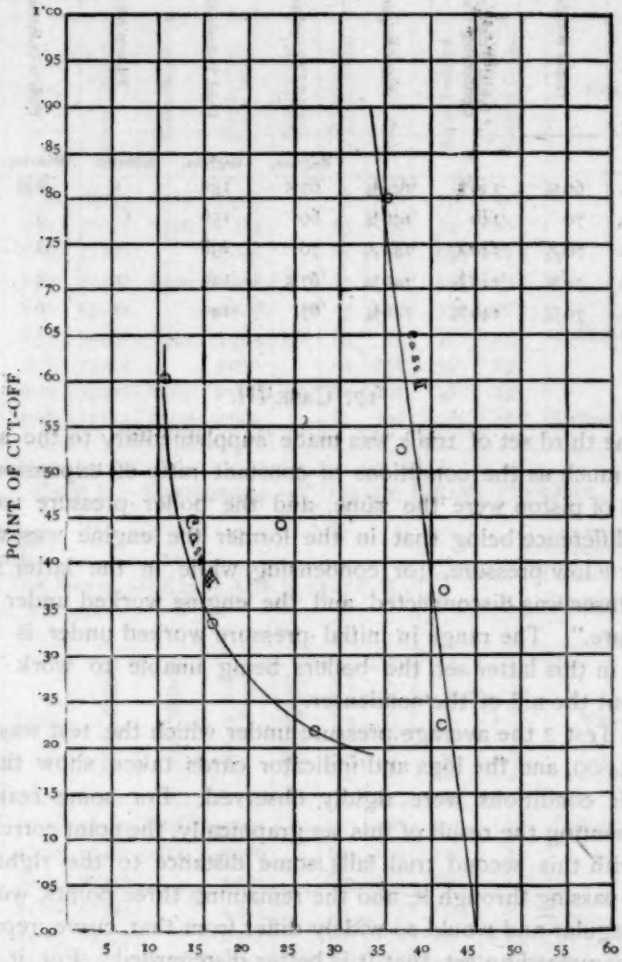
17. CASE III.

The third set of trials was made supplementary to the second, in as much as the conditions of constant ratio of expansion, and speed of piston were the same, and the boiler pressure variable. The difference being that in the former the engine was worked under "low pressure," or condensing, while in the latter set the condenser was disconnected and the engine worked under "high pressure." The range in initial pressure worked under is not as great in this latter set, the boilers being unable to work higher without the aid of the condenser.

In Test 2 the average pressure under which the test was made was 44·09, and the logs and indicator cards taken show that the proper conditions were rigidly observed. For some reason, in representing the result of this set graphically, the point corresponding with this second trial falls some distance to the right, so a curve passing through it, and the remaining three points, would be so irregular and would so widely differ from that curve representing the preceding set, that it is better disregarded. For, it can be readily understood, that the curve representing each should have the same general appearance, although a difference in the absolute amounts of condensation in the two cases would be expected and are seen to exist. *Plate IV.*

The logs and calorimeter tests made during this set are given below:

PLATE IV.



CONDENSATION WITH VARYING PRESSURE.

CASE III.—NON-CONDENSING.—VARIABLE BOILER PRESSURE.

Table No. 3.
Test No. 10.
Date of Test, May 27, 1884, 1·50 to 4·50 P. M.
Duration, 3 Hours.

Conditions: { Variable Pressure, Constant Speed, Constant Cut-off.

Time.	Speed Counter.	Water Meter.	Boiler Pressure.	Temperature Feed Water.	Remarks.	Calorimeter Test.
Start,						
1·50	905	6688·3	20·	102		Initial weight, ... 71⅛
2·05	1915	6696·	22·	105		Initial w'ht + W. lbs. of cond. water, 141⅛
2·20	2909	6705·5	21·	110		Final weight after introducing steam, 146⅞
2·35	3910		21·	112	Deduct 23 3-16 lbs.	Initial temperature, ... 71· ° F.
2·50	4905		24·			Final temperature, ... 152·5° F.
3·05	5901		21·5			Boiler pressure, ... 22
3·20	6899		22·	112		Flow of steam in air, ... 1 min.
3·35	7891	6744·	23·25	111	Deduct 23 3-16 lbs.	Flow of steam in calorimeter, . . . 6 min.
3·50	8889	6750·5	20·			
4·05	9886		20·5	96		
4·20	10884		23·25	94		
4·35	11881		21·5	95	Deduct 27 4-16 lbs	
4·50	12878	6779·	22·			
Finish						

CASE III.—NON-CONDENSING.—VARIABLE BOILER PRESSURE.

Table No. 3.
Test No. 11.
Date of Test, May 28, 1884, 7·30 to 10·30 A. M.
Duration, 3 Hours.

Conditions: { Variable Pressure, Constant Speed, Constant Cut-off.

Time.	Speed Counter.	Water Meter.	Boiler Pressure.	Temperature Feed Water.	Remarks.	Calorimeter Test.
Start,						
7·30	12520	6854·	72·			Initial weight, ... 71. lbs.
7·45	13537	6858·5	69·	158	Deduct 22½ lbs.	Initial w'ht + condensing water, . 141· lbs.
8·00	14562	6879·	55·	116		Final weight, ... 146¼ "
8·15	15585	6905·	59·	106	Deduct 23½ lbs.	Initial temperature, ... 68· ° F.
8·30	16615	6908·5	61·	116		Final temperature, ... 146· ° F.
8·45	17624	6921·	62·			Boiler pressure, ... 59
9·00	18636	6935·	51·	93		Steam flowed in air, ... 1 min.
9·15	19680		62·			Steam flowed in calorimeter, ... 3 min.
9·30	20685	6966·	59·5	104	Deduct 21¾ lbs.	
9·45	21701	6980·	60·	140		
10·00	22716	6991·	61·	98		
10·10	23738	7014·	56·5			
10·30	24757	7037·	61·	84	Deduct 23¾ lbs.	
Finish						

CASE III.—NON-CONDENSING.—VARIABLE BOILER PRESSURE.

Table No. 3.
Test No. 12.
Date of Test, May 28, 1884, 11·08 A. M. to 1·38 P. M.
Duration, 2½ Hours.

Conditions: { Variable Pressure. Constant Speed. Constant Cut-off.

Time	Speed Counter.	Water Meter.	Boiler Pressure.	Temperature Feed Water.	Remarks.	Calorimeter Test.
Start,						
11·08	25345	7078·2	44·			
11·23	26410	7086·	43·	94	Deduct 23⅛ lbs.	Initial weight, ... 71 1-16 lbs.
11·38	27417	7109·	45·	98		Initial weight + cond. water, . 141 1-16 lbs.
11·53	28448	7113·	43·	98		Final weight, ... 146 1-16 lbs.
12·08	29510	7126·	44·	98		Initial temperature, ... 65° F.
12·23	30557	7142·	44·	98		Final temperature, ... 137° F.
12·38	31583	7150·	43·	86		Boiler pressure, ... 43·5
12·53	32586	7169·	45·	94		Flow in air, ... 1 min.
1·08	33604	7178·	44·	100	Deduct 25½ lbs.	Flow in calorimeter, ... 5 min.
1·23	34627	7193·	44·5	100		
1·38	35631	7215·	45·5	88	Deduct 16 lbs.	
Finish						

CASE III.—NON-CONDENSING.—VARIABLE BOILER PRESSURE.

Table No. 3.
Test No. 13.
Date of Test, May 28, 1884, 2·19 to 5·19 P. M.
Duration, 3 Hours.

Conditions: { Variable Pressure. Constant Speed. Constant Cut-off.

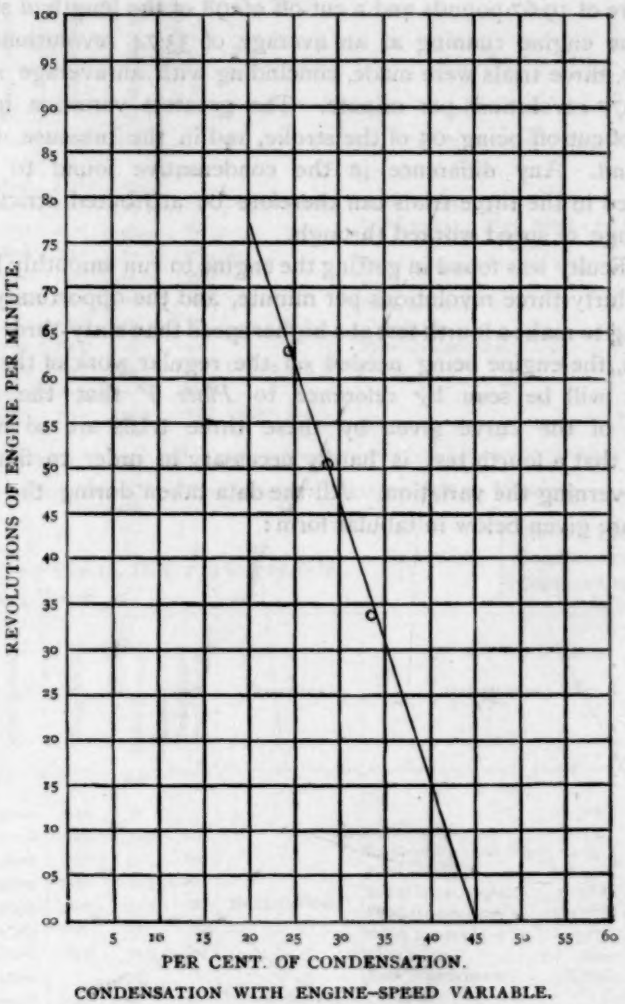
Time	Speed Counter.	Water Meter.	Boiler Pressure.	Temperature Feed Water.	Remarks.	Calorimeter Test.		
Start,								
2·19	36204	7224·	32·				1st.	2d.
2·34	37198	7233·	34·5	100		Initial weight, ...	71¼	71 9-16
2·49	38207	7242·	35·	106		In. weight + cond. water,	141¼	141 9-16
3·04	39225	7252·	34·5	110		Final weight, ...	148 11-16	148
3·19	40253	7265·	33·5	86	Deduct 18 lbs.	Initial temperature, . . .	60·5° F.	60.5° F.
3·34	41265	7270·	33·	88		Final temperature, . . .	160·5° F.	150.5° F.
3·49	42280	7292·	32·5	102		Boiler pressure, ...	33·5	33·
4·04	43294		34·			Flow in air, ...	1 min.	1 min.
4·19	44300	7303·	32·	102	Deduct 20 3-10 lbs.	Flow in calorimeter, . .	5½ min.	5 min.
4·34	45311	7313·	32·5	105				
4·49	46325	7322·	34·	104				
5·04	47325	7331·	34·5	90				
5·19	48357	7343·5	33·5	95	Deduct 24⅝ lbs.			
Finish								

18. Case IV.

The fourth and last set made were to determine the effect caused by different speed of engine or time of exposure of the initial surface of the cylinder. Starting with an average boiler pressure of 19·67 pounds and a cut-off of ·98 of the length of stroke and the engine running at an average of 33·74 revolutions per minute, three trials were made, concluding with an average speed of 62·977 revolutions per minute. The greatest variation in the point of cut-off being ·05 of the stroke, and in the pressure ·63 of a pound. Any difference in the condensative found to have occurred in the three trials can therefore be attributed strictly to the range of speed worked through.

Difficulty was found in getting the engine to run smoothly lower than thirty-three revolutions per minute, and the opportunity was wanting to make a fourth test at a higher speed than sixty-three revolutions, the engine being needed on the regular work of the mill. But it will be seen by reference to *Plate V* that the three points of the curve given by these three trials are so nearly in line that a fourth test is hardly necessary in order to find the law governing the variation. All the data taken during the three trials are given below in tabular form:

PLATE V.



CONDENSATION WITH ENGINE-SPEED VARIABLE.

CASE IV.—CONDENSING.—VARIABLE REVOLUTIONS.

Table No. 4.
Test No. 14.
Date of Test, May 29, 1884, 7·57 to 9·42 A M.
Duration, 1¾ hours.

Conditions: { Variable Speed. Constant Cut-off. Constant Pressure.

	Time.	Speed Counter.	Water Meter.	Height over Notch Board.	Temperature Feed Water.	Temperature Injection Water.	Temperature Weir.	Boiler Pressure.	Vacuum Gauge.	Remarks.
Start,	7·57	1182	7380·	1·900		53·	130	21·	20·	
	8·07	1497	7404·5	2·280	166	53·	135	21·	20·	
	8·17	1768		2·212		52·5	123	19·	20·	
	8·27	2121	7405·	2·282	110	53·	138	19·	20·	
	8·37	2478		2·280		53·	134	20·	20·	
	8·47	2819		2.288		53·	130	19·5	20·	
	8·57	3162		2·3		52·	137	19·	20·	
	9·07	3532		2·298	100	52·	137	20·	20·	$23\frac{3}{16}$ lbs. out.
	9·17	3880	7438 2	2·290		52·5	132	19·5	20·	
	9·27	4215		2·256		52·	131	19·	20·	
	9·37	4556		2·308	80	52·	134	20·	20·	
Finish,	9·42	4725	7446·5	2·312		52·	128	19·	20·	Deduct 7 lbs.

CASE IV.—CONDENSING.—VARIABLE REVOLUTIONS.

Table No. 4.
Test No. 15.
Date of Test, May 29, 1884, 9·47 to 11·47 A. M.
Duration, 2 Hours.

Conditions: { Variable Speed. Constant Cut-off. Constant Pressure.

	Time.	Speed Counter.	Water Meter.	Height over Notch Board.	Temperature Feed Water.	Temperature Injection Water.	Temperature Weir.	Boiler Pressure.	Vacuum Gauge.	Remarks.
Start.	9·47	4949	7446·5	2·780		52·	129	18·5	21·5	
	9·57	5431	7452·	2·774	90	52·5	130	19·	22·	
	10·07	5927		2·712		52·5	135	18·5	21·5	
	10·17	6437	7466·	2·718		52·5	136	20·5	21·	
	10·27	6925		3·040	110	52·5	120	19·5	22·5	Deduct 18 lbs.
	10·37	7421	7470·	3·100		52·5	120	19·	22·5	
	10·47	7927		3·106		52·5	117	16·	23·	
	11·07	8975		3·036		53·	122	19·	22·5	
	11·17	9496	7507·	3·085	90	52·5	119	18·	22·5	
	11·27	9990	7514·	3·085		52·5	123	20·	22·5	
	11·37	10495	7520·	3·085		52·5	120	19·5	22·5	Deduct $17\frac{1}{16}$ lbs.
Finish,	11·47	10985	7543·	3·085	80	53·	123	21·	22·5	

Case IV.—Condensing.—Variable Revolutions.

Table No. 4.
Test No. 16.
Date of Test, May 29, 1884, 3·00 to 4·30 P. M.
Duration, 1½ Hours.

Conditions: Variable Speed. Constant Cut-off. Constant Pressure.

	Time.	Speed Counter.	Water Meter.	Height over Notch Board.	Temperature Feed Water.	Temperature Injection Water	Temperature Weir.	Boiler Pressure.	Vacuum Gauge.	Remarks.
Start,	3·00	18886	7655·5	4·154	98	55	105	24·	24·	
	3·10	19491	7668·	4·100		54	104	21·	23·5	
	3·20	20059	7672·	4·200	104	54	104	20·5	23 5	
	3·30	20639		4·140	100	54	103	20·	23·5	
	3·40	21248		4·040	86	54	107	17·5	23·5	Deduct 20¼ lbs.
	3·50	21925		4·200		54	107	18·	23·75	
	4·00	22597		4·168		54	102	17·5	23·5	
	4·10	23257		4·100	103	54	108	17·	23·75	
	4·20	23911		4·262		54	101	16·	23·5	
Finish,	4·30	24554	7740·	4·080		54	112	19·	23·75	Deduct 5 lbs.

Condensing Tests.

Case IV.—Calorimeter Trials.—Variable Revolutions.

In Connection with Engine Trial.	Initial Weight.	Initial Weight plus Condensing Water.	Final Weight.	Initial Temperature.	Final Temperature.	Flow in Air.	Flow in Calorimeter.	Boiler Pressure.
				degrees.	degrees.	minutes.	minutes.	
No. 14.	$71\frac{7}{16}$	$141\frac{7}{16}$	$148\frac{1}{16}$	55·	145·5	1	7	20·
No. 15.	$71\frac{5}{8}$	$141\frac{5}{8}$	$147\frac{1}{8}$	55·5	142·5	1	7	20·5
No. 16.	$71\frac{3}{4}$	$141\frac{3}{4}$	$145\frac{5}{8}$	55·75	113·	1	5	145·

(*To be Continued.*)

RECENT IMPROVEMENTS IN THE MANUFACTURE OF STEEL.*

It may be of interest to the readers of THE JOURNAL to have more definite knowledge, than they can gather from newspapers, of the new processes which are being introduced for the manufacture of steel.

The depressed condition of the iron and steel trade has stimulated inventors and manufacturers to renewed efforts to lessen the cost of production, and the result of these efforts is a number of new processes that have recently been developed, alike in principle, but differing materially in practical application.

The four processes that are exciting the most attention, are the Clapp-Griffiths, the Davy, the Gordon and the Avesta.

They are all based on the pneumatic or Bessemer process, and differ only in the manner of using or applying the blast or tuyeres. The most important and well-developed of the above processes is the Clapp-Griffiths, and the first plant built in this country, at the works of Messrs. Oliver Brothers & Phillips, has been in successful operation for the last six months.

Notwithstanding the unfavorable comments that have been passed upon this process by some of the metallurgical savants of the day, I look upon it as the beginning of a successful revolution against the primitive and barbarous method of puddling, and as inaugurating the most important epoch in the history of iron since the advent of the Bessemer process. It is interesting and instructive to think that the aim of the Clapp-Griffiths process is to accomplish exactly the same result that Bessemer attempted thirty years ago, or in 1855. Bessemer's idea, and the one that came very near wrecking him and thus depriving the world for a

* To make a complete *résumé* of the subject, Mr. Salom, by request, has elaborated the details of a number of the more prominent modern processes of steel manufacture, which could only be incidentally referred to in his lecture. The same is here introduced as supplementary to his lecture on "The Metallurgy of Steel," published in the September issue of THE JOURNAL.—[Committee on Publication.]

time at least of his invaluable discovery, was to produce cast wrought iron. It is true that Bessemer expected to produce all grades of cast steel, from the hardest tool steel to the softest open-hearth boiler plate, by interrupting the operation at different stages of decarbonization. This expectation was not realized (although it bids fair to be in the future) owing to the presence of silicon and

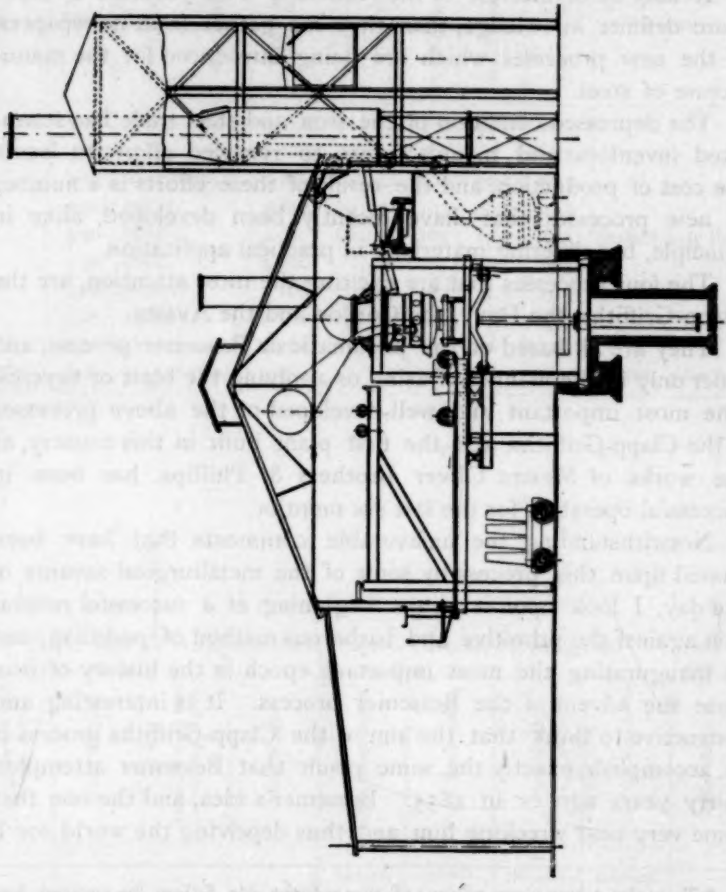
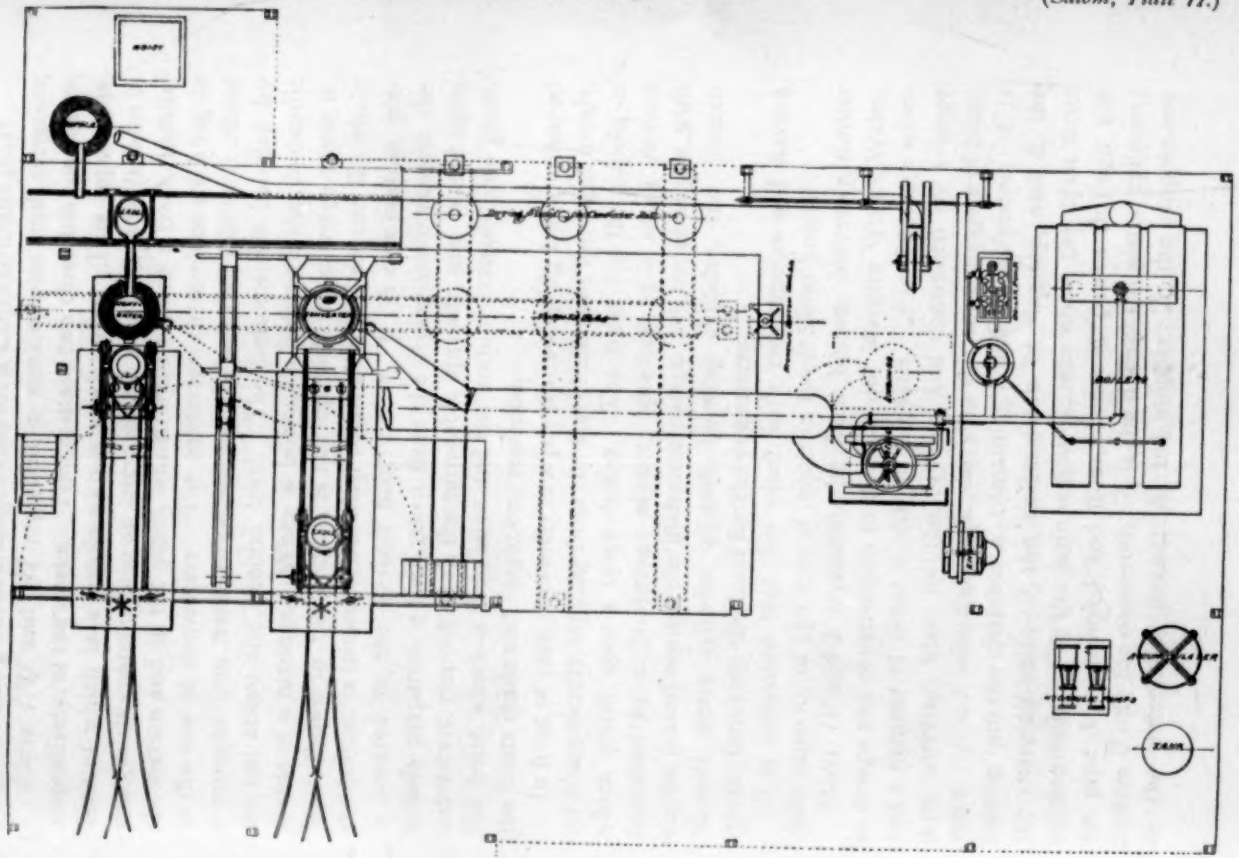


PLATE I.—Clapp-Griffiths Process. (Section of works.)

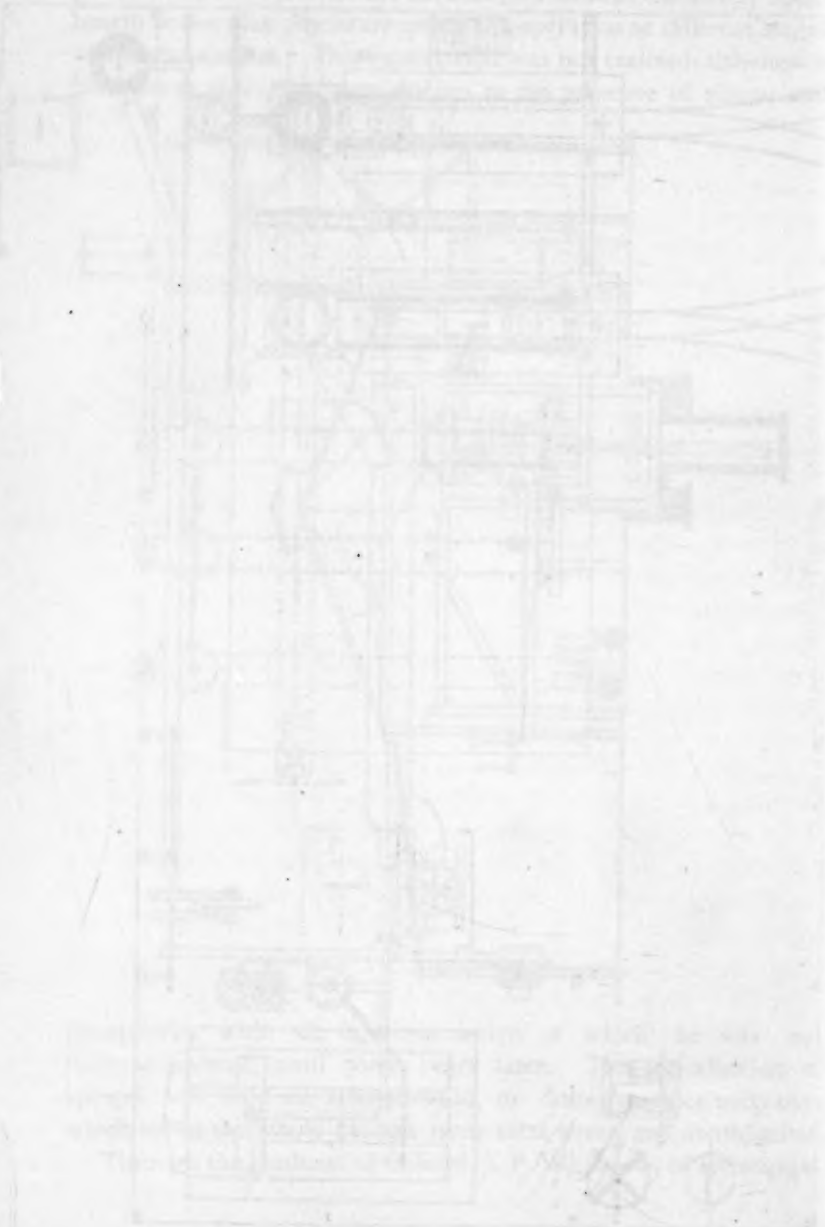
phosphorus, with the injurious action of which he was not fully acquainted, until some years later. The introduction of spiegel was only an after-thought, or rather an after-necessity, which saved the whole process from total wreck and annihilation.

Through the kindness of Colonel J. P. Witherow, of Pittsburgh,



Clapp-Griffiths Process. (Plan of works.)

PLATE I.—Clapp-Griffiths Process. (Section of works.)



who, with the Messrs. Oliver, controls the patents for this country, I am able to give the readers of THE JOURNAL some idea of the converter and general arrangements for a Clapp-Griffiths plant.

Plates I, II and *III* represent a section, plan and elevation, respectively, of the works. They contain two three-ton converters, back of which is a cupola with a stock hoist. This cupola discharges its contents into an intermediate weighing ladle (shown in the section and in the plan), which travels along a track parallel to the row of converters The converters themselves are hung in a wrought iron frame-work, making the working platform about ten feet above the general level. The bottoms are handled by means of a carriage. When a bottom is defective, the hydraulic hoist, shown in the section, is run up with the carriage upon it, the bottom is loosened, the blast connection broken, and the whole is lowered to the ground floor. * * * A new bottom, previously prepared, is then taken from the drying-stove nearest the converters, run out upon the turn-table opposite, and placed upon the hoist, which is then lifted and the bottom fastened into place, the joints made and operations resumed.

In front of each converter is a peculiar swinging track, arranged for conveniently tapping from the steel ladle into the ingot molds, which stand upon a truck below. The blast for the cupola is furnished by a Sturtevant blower, operated by a small vertical engine placed nearest the hydraulic pumps. A larger engine, with 16-inch steam cylinder, 48-inch blowing cylinder, and 30-inch stroke, furnishes the blast for the converters.

It is estimated that this plant, with two cupolas, will have a daily capacity of 150 tons of ingots in twenty-four hours.

Plates IV and *V* represent the fixed bottom converter, similar in design and construction to those at the Margrin Works, Wales, and a number of other works in England. *A* is the main wind-p pe, carrying blast for the tuyeres, and controlled by a large valve. *B* is a subsidiary pipe, carrying the same pressure of blast, leading into the chamber *G*, containing a differential piston. *C* is the charging-hole; *D*, the cinder-notch or slag-top, and *E*, the tapping-hole. *F* is the main wind-box, into which the blast from the pipe *A* is conveyed, and from whence it passes through the tuyeres *O* into the converter. *G* is the differential piston cylinder; *H*, the differential piston; *K*, the stopper; *L*, the stopper-rod

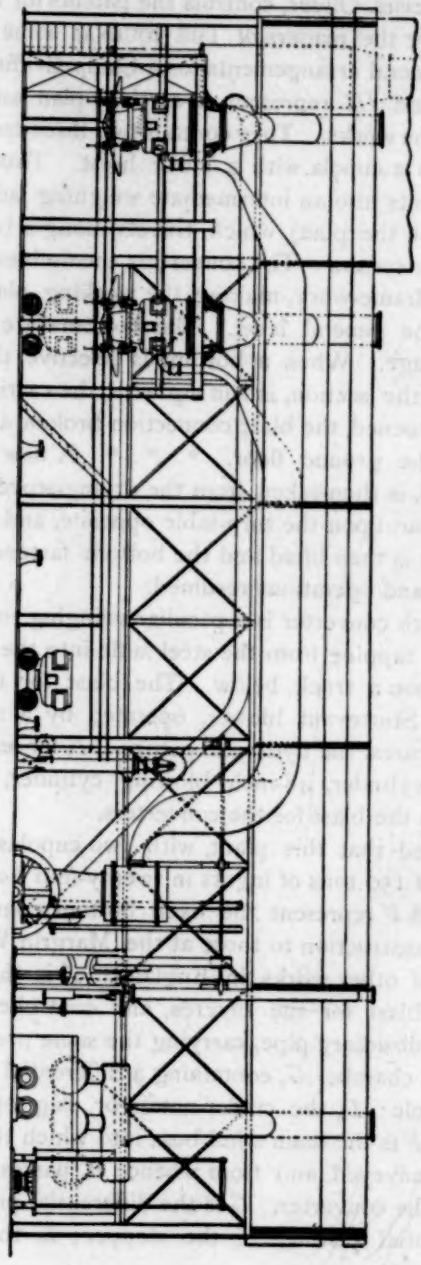


PLATE III.—Clapp-Griffiths Process. (Elevation of works.)

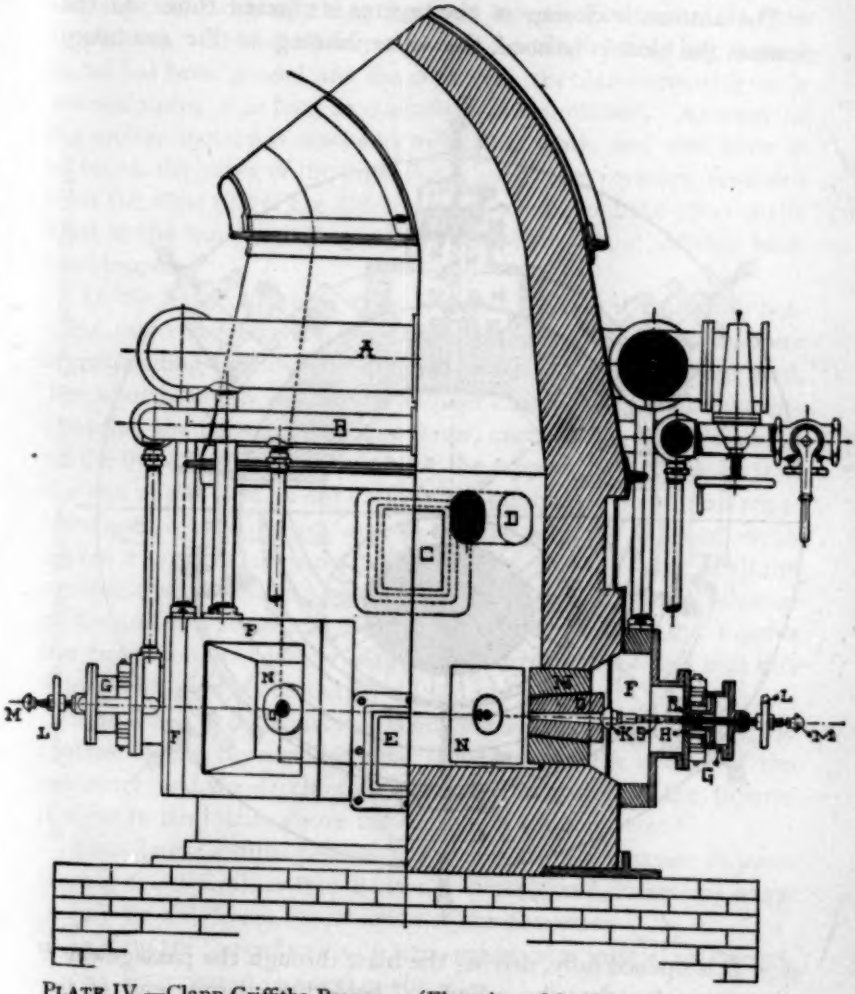


PLATE IV.—Clapp-Griffiths Process. (Elevation of fixed-bottom converter.)

hand wheel; *M*, peep-hole cap; *N*, the tuyere block, and *O*, the tuyeres. *P* is a passage from down-take from the pipe *B* to the back of the differential piston *H*. *R* is the stopper-rod adjusting screw, and *S* the stopper-rod.

The automatic closing of the tuyeres is effected thus: At the moment the blow is finished, the valve leading to the secondary

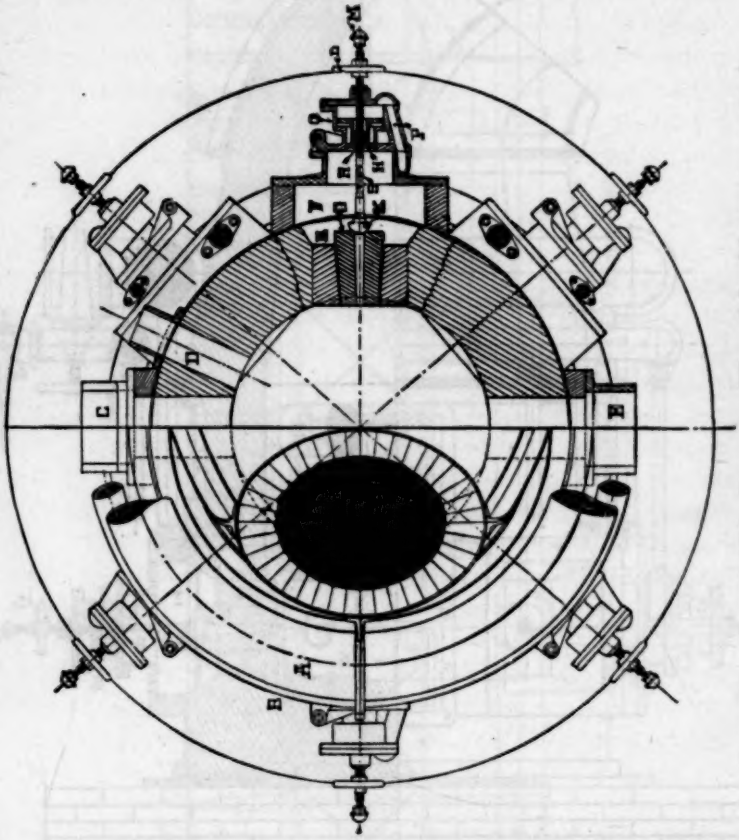


PLATE V.—Clapp-Griffiths Process. (Plan of fixed-bottom converter.)

pipe *B* is opened fully, driving the blast through the passageway *P* (shown on plan) into the cylinder *G* behind the larger end of the piston *H*. The pressure of blast being the same in the cylinder *G* as in the wind-box *F*, as the piston is double faced, having one end of considerably larger diameter than the other, the same pressure of blast per square inch, introduced into the chamber *G*, forces the

piston and stopper rod against the end of the tuyere *O*, closing the hole therein except for a small aperture in the stopper *K*, fed by the holes shown through the hollow stem *S*. This small amount of blast suffices to keep the molten metal from running back into the tuyeres, keeping them free and open. The metal is then tapped out. This operation is simply reversed when the molten metal has been poured into the converter, the blast remaining on in the main-pipe *A* as long as operations are continued. As soon as the molten metal has reached a sufficient depth, and the blow is to begin, the valve of the pipe *B* is closed, the pressure removed from the blast end of the differential piston *H*, and the effect of the blast in the wind-box *F* is to drive the piston *H* out, driving back the stopper.

In the new converters, *Plates VI* and *VII*, with removable bottoms, this arrangement was found undesirable. No pipes whatever corresponding to the pipes *A* and *B* of the old converters are used. The blast is simply led into an annular chamber or wind box surrounding the bottom of the converter, except for a short distance on the front side, to allow room for the tapping hole. The blast at the end of the blow is not entirely cut off, but its pressure is regulated and reduced by any one of several patented devices, which makes it possible to keep a uniform pressure at the noses of all the tuyeres, thus accomplishing the same purpose as the differential piston and stopper of the old converter, viz., to keep the tuyeres free from molten metal or slag. In practice, it is found that this works with perfect certainty.

The lining of the converter is gannister, twelve inches thick, the centres of the tuyeres are nine inches above the bottom of the converter, and the depth of metal above the centre of the tuyeres is eight to ten inches above the bottom of the converter.*

The Clapp-Griffiths process has succeeded in a measure in overcoming the difficulties that Bessemer experienced, in several ways.

(1.) By a different construction of the converter.

(2.) By the introduction of ferro-manganese instead of spiegel.

(3.) By a partial removal of the slag.

(4.) By the introduction of what might be termed a new principle, viz., the change in position and character of the tuyeres,

*Abridged from Hunt's paper on "The Clapp-Griffiths Process." Trans. Am. Inst. Mining Engineers. Vol. XIII.

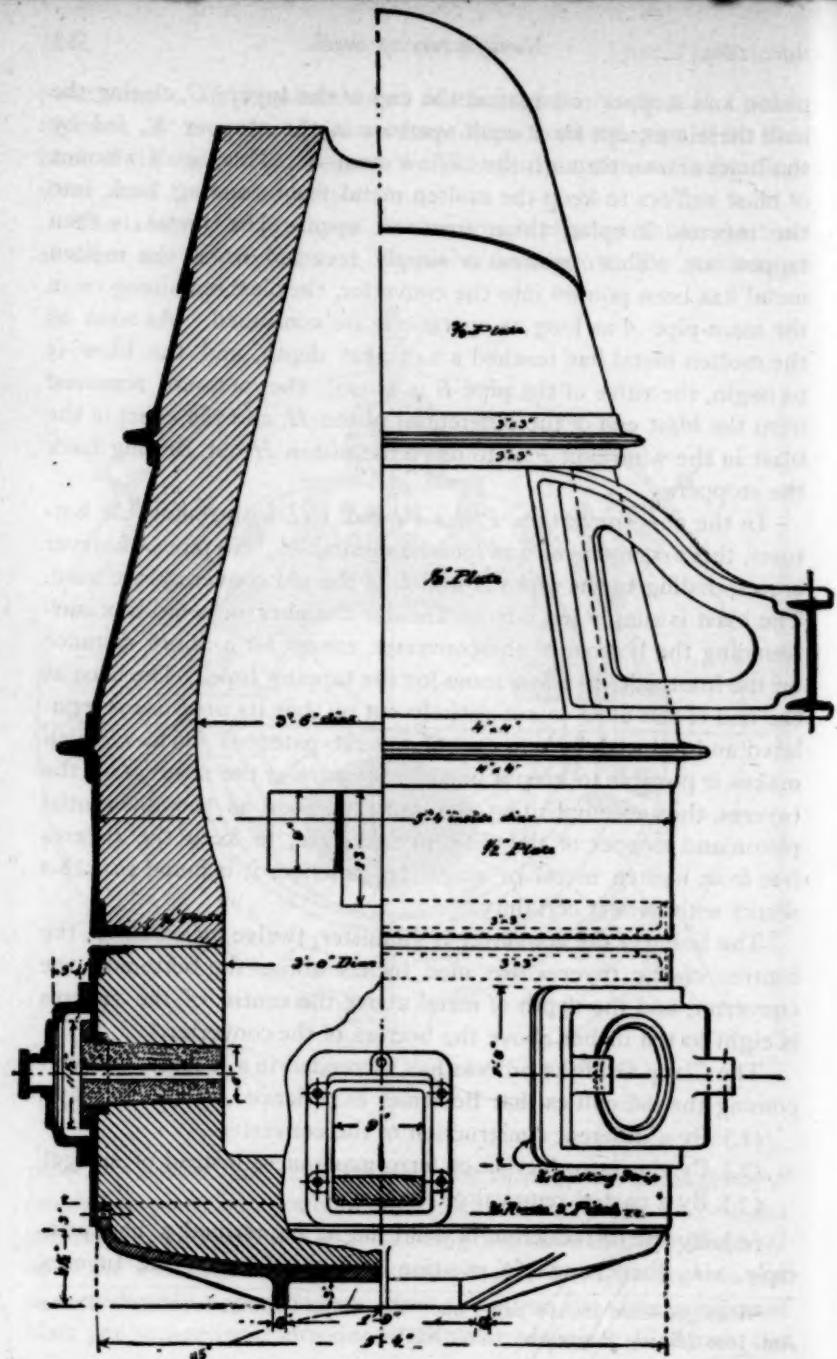


PLATE VI.—Clapp-Griffiths Process. (Sectional elevation of removable-bottom Clapp-Griffiths converter.)

involving as it does a different chemical reaction in the converter.

It will not do, therefore, for the Bessemer men to sneer at their little antagonist. The facts remain that the Bessemer men have been so busy making rails that they have never done anything towards the solution of the problem of avoiding puddling. It has only been within the last few years, that the demand for rails falling short of

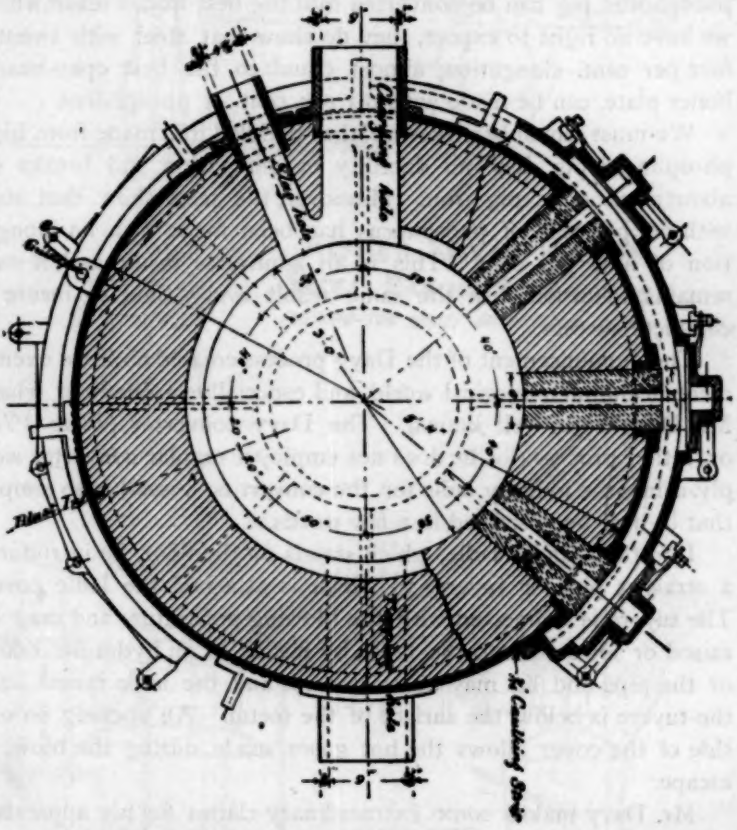


PLATE VII.—Clapp-Griffiths Process. (Section of removable-bottom converter.)

the possible products of the Bessemer mills, they have endeavored, to some extent, to increase the quality of their product, so as to successfully compete with open-hearth steel and charcoal iron. The result of these attempts has not been altogether satisfactory, but enough has been done to show that the highest qualities that steel possesses can sometimes be obtained by the Bessemer process, and it only needs a more careful study of the conditions, both

physical and chemical, under which this material is obtained to be able to produce it with gratifying regularity.

I believe, however, that these results can be more easily obtained in working on a smaller amount of metal than is customary in a Bessemer works.

While the results thus far published do not show that high phosphorus pig can be converted into the best iron, a result which we have no right to expect, they do show that steel with twenty-four per cent. elongation, almost equal to the best open-hearth boiler plate, can be made with 0·3 per cent. of phosphorus.

We must remember, likewise, that puddled iron made from high phosphorus pig has no ductility or elongation and breaks off about as short as pig iron. Moreover, the tests show that steel with 0·5 per cent. of phosphorus has been made with an elongation of nine per cent. This is an admirable result and it only remains to accomplish the same result every time, to insure a complete success.

The announcement of the Davy process created no little excitement in the metallurgical world, and especially in Sheffield, where Mr. Davy is so well known. The Davy converter, (*Plate VIII*) or, rather process (for he does not employ a regular converter simply using the foundry ladle for the converting vessel) is so simple, that it may be described in a few words.

Into the foundry ladle, which stands on the floor, he introduces a straight pipe or tuyere, to which is attached the ladle cover. The tuyere is connected with the blowing apparatus, and may be raised or lowered into the ladle, by means of an hydraulic crane, or the pipe and lid may be stationary, and the ladle raised until the tuyere is below the surface of the metal. An opening on one side of the cover allows the hot gases, made during the blow, to escape.

Mr. Davy makes some extraordinary claims for his apparatus. He claims to be able to make steel with 80,000 pounds tensile strength, and twenty per cent. elongation; that such steel will not cost more than from $20 to $22 per ton, taking pig iron at $15; and that the cost of the apparatus for producing one ton at a time would only be $3,000 complete.

The almost ridiculous simplicity of the Davy process is a strong argument in its favor. The possibility of being able to make all

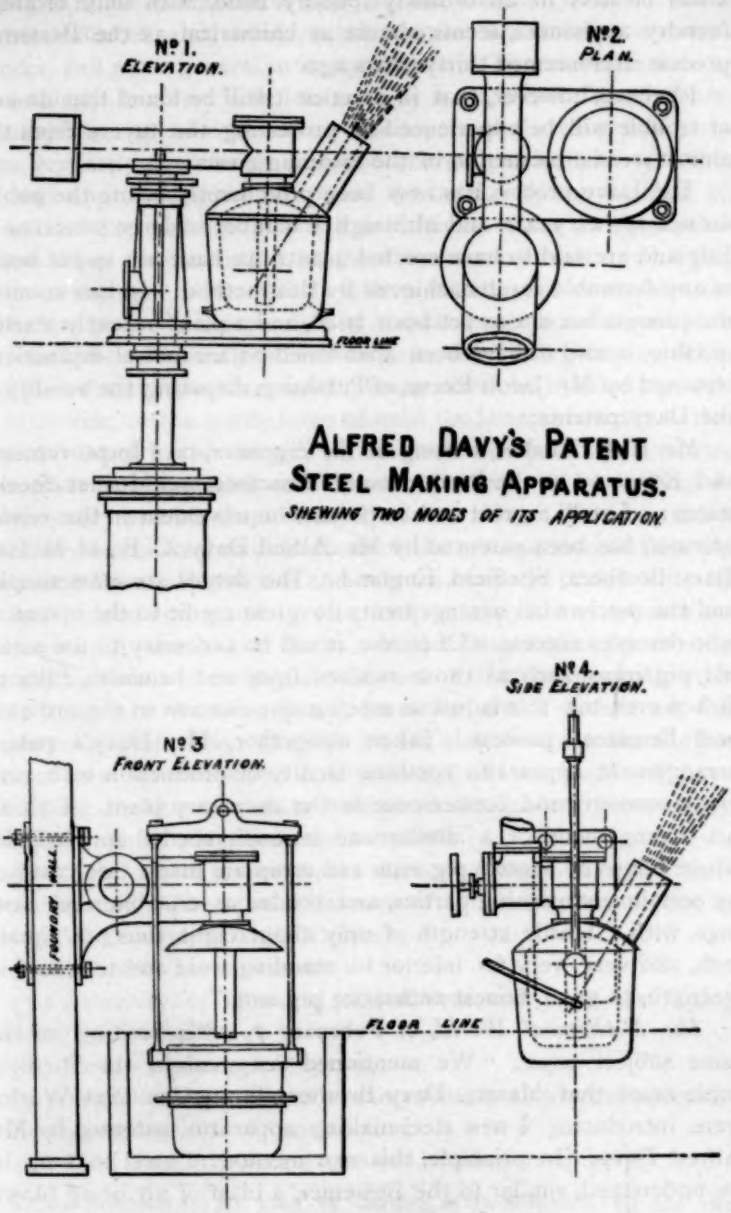

Nº1.
ELEVATION.
Nº2.
PLAN.
FLOOR LINE
ALFRED DAVY'S PATENT
STEEL MAKING APPARATUS.
SHEWING TWO MODES OF ITS APPLICATION.
Nº4.
SIDE ELEVATION.
Nº3
FRONT ELEVATION.
FOUNDRY WALL
FLOOR LINE

PLATE VIII.

kinds of steel in an ordinary foundry ladle, with only ordinary foundry appliances, seems almost as chimerical as the Bessemer process itself seemed thirty years ago.

I believe, however, that in practice it will be found that no end of trouble will be experienced in protecting the tuyere from the almost irresistible action of the escaping gases.

The Davy process has now been prominently before the public for nearly two years, and although a number of large concerns in England are said to have erected plants, we have not as yet heard of any favorable results achieved by this method. In this country the process has not as yet been tried, and a plant recently started in Ohio, is said to have been abandoned on account of injunctions obtained by Mr. Jacob Reese, of Pittsburg, disputing the validity of the Davy patents.

Mr. R. F. Mushet, writing in the *Engineer*, on "Improvements and Economy in the Production of Bessemer and Mushet Steel," states: "Lately a great and important improvement in the *modus operandi* has been patented by Mr. Alfred Davy, C. E., of Messrs. Davy Brothers, Sheffield, England. The details are most simple, and the mechanical arrangements do great credit to the inventor, who deserves success. Of course, it will be necessary to use suitable pig irons, such as those smelted from red hematite, Elba or Bilboa ores, but this is just as much a *sine qua non* in the ordinary acid Bessemer process. Taken altogether, Mr. Davy's patent arrangement appears to combine facility of production with very great economy and convenience in the necessary plant. I think his arrangement or a similar one is much needed in Sheffield, where alloys of Scotch pig iron and scrap are made into castings by certain enterprising parties, and vended as crucible steel castings, with a tensile strength of only about eight tons per square inch, and very, very far inferior for standing wear and tear, and in strength, to good, honest anthracite pig iron."

The Mechanical World, of February 7, 1884, writing on the same subject, says: "We mentioned last week in our Sheffield trade notes that Messrs. Davy Brothers, limited, of Park Works, were introducing a new steel-making apparatus, patented by Mr. Alfred Davy. In principle, this new method of steel making is, we understand, similar to the Bessemer, a blast of air being blown into and thus used to decarbonize and purify molten cast iron.

"The mode and means of applying the blast is, however, altogether different, and, indeed, much simpler under many circumstances, and more general in its application. The converter is like an ordinary foundry ladle, covered by a lid, which has a short spout projecting from its side. The tuyere of the blast pipe projects vertically downwards into the charge, passing through the lid, and approaching the bottom of the ladle. When the blast is turned on the gaseous products are driven out at the spout, the action being similar to that in the Bessemer converter. But the great advantage in Mr. Davy's system is that the converters or ladles are perfectly independent of the blast pipe connections, and can easily be taken with either a large or small charge at any time, and placed under the blast nozzle, being raised by hydraulic power or otherwise, or the nozzle lowered until the latter is inserted to the required depth in the charge. It does not therefore require extensive plant to make steel, and it is not necessary to be making it continuously as with the ordinary Bessemer plant. We understand Mr. Davy has several modifications of the system, more or less on the above lines. In practice and over an extended trial, it is stated to have given very good results, and certainly there appears no reason why it should not do so. If Mr. Davy opens up to iron founders a means of making either steel or iron castings with little more than iron founding plant, he will confer a benefit on the engineering profession. Perhaps he can also show the users of his patents how to make sound steel castings. If so, much will have been done, for those much-to-be-desired articles are yet a rarity even in the most advanced establishments."

The third recent improvement attracting attention, is the Gordon converter. The special features of this converter are due to Mr. F. W. Gordon, of Philadelphia.*

He claims for his converter:

(1.) Intensity of chemical action removed from the lining insuring its durability.

(2.) Ready replacing of tuyeres.

(3.) Withdrawing tuyeres instantaneously and simultaneously, without complications of construction.

(4.) Mild blast, avoiding expensive blowing machinery.

* I am indebted to Mr. Fred. W. Gordon, of Philadelphia, for the cuts representing his converter, and also for the explanation of the same. P. G. S.

(5.) Reduction of carbon and silicon to any desired extent, even to totality.

(6.) Ready access for repairs to every part.

(7.) Low cost of construction and maintenance.

(8.) Production of the best steel from high grade pig irons.

(9.) Bessemer steel from ordinary pig irons.

(10.) Phosphorus steel from common pig irons.

(11.) Chilling irons, malleable irons, strong machinery irons, semi-steel and steel castings from ordinary grades of mineral fuel pig irons.

The special features of Gordon's converter are the durability of the sides and bottom, and the tuyere, which dips into the bath through side-holes piercing the shell, and is moved into and out of the metal, automatically, by the action of the blast.

Referring to the accompanying illustrations: (*Plates IX to XII.*)

A, is a fire-clay tuyere.

B, is a gas pipe, turned on its upper end, the turned end fitting neatly into the holder *C*, but not secured thereto.

C, tapered tuyere holder, fitted into the piston *E*, secured by a double bayonet catch.

D, tapered wrought sleeve, threaded on to *C*, gripping the tapered end of *A*.

The holder *C*, sleeve *D* and pipe *B*, are to be in duplicate and adjusted to each clay tuyere ready for insertion when the cylinder and tuyere are thrown into position, as shown in dotted lines.

E, small piston, preventing the free escape of the blast, forming hold fast for *C*.

F, openings for the passage of the blast into the tuyeres.

G, piston for raising or lowering the tuyeres.

H, valve, seating at *I*, the stem of which plays through the head of cylinder at *J*.

K, handle of cram screw, used to fix the valve stem at any point of its downward stroke.

M, conduit, leading from upper part of cylinder.

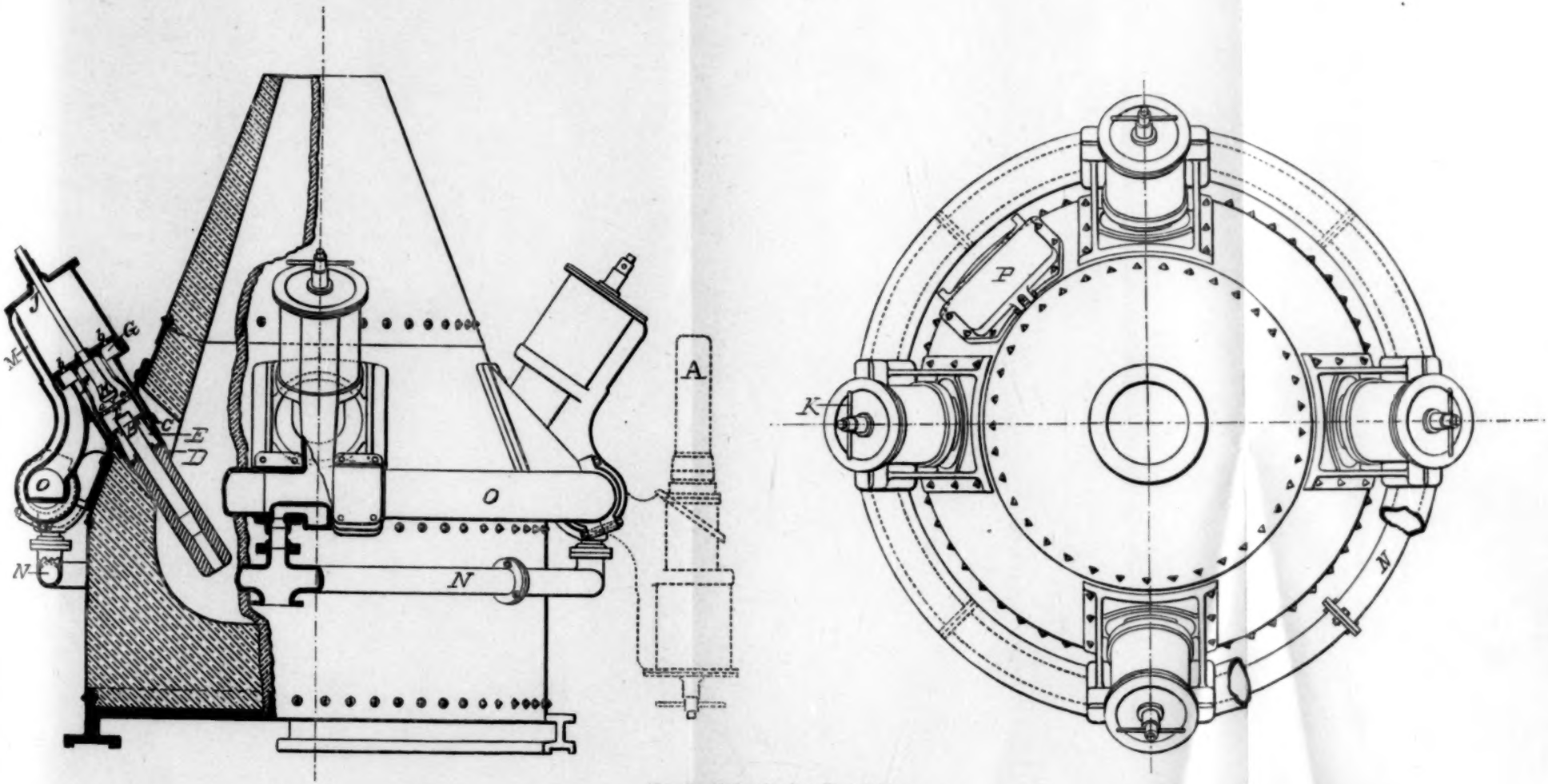
N, a discharge pipe, common to all the tuyeres, having a single exhaust valve.

O, main blast pipe.

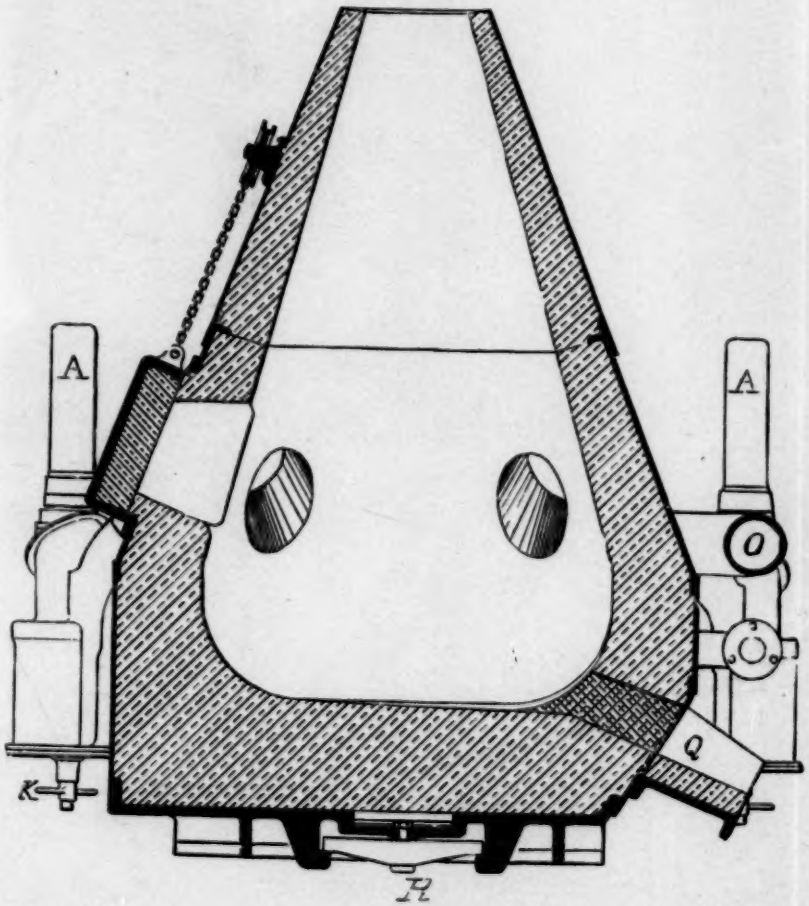
P, charging door.

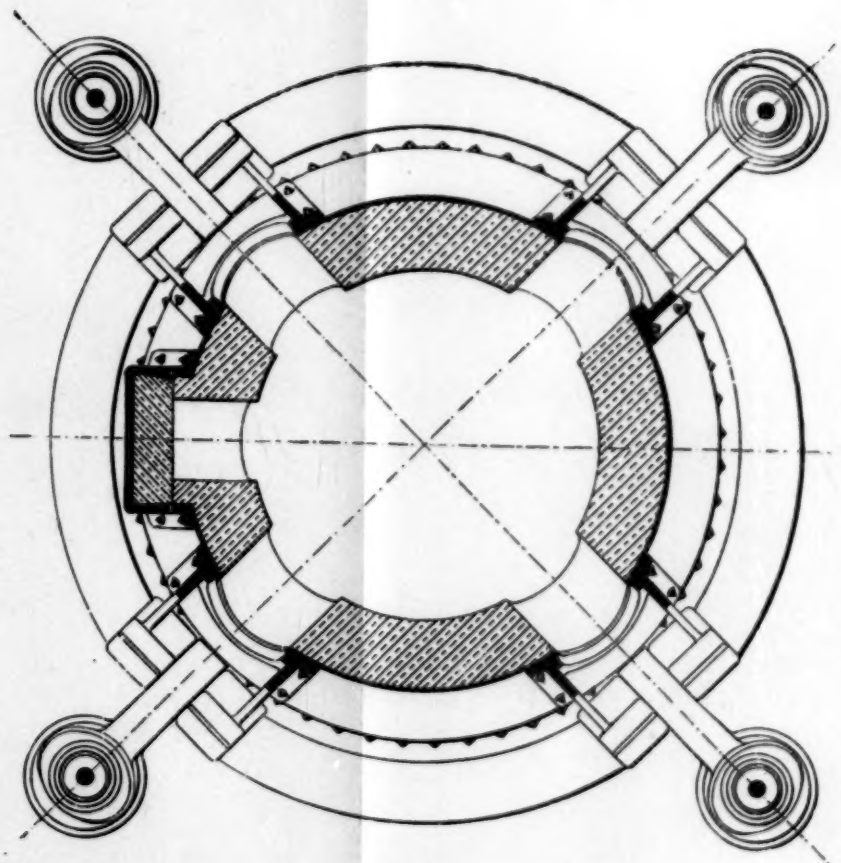
Q, tapping hole.

R, cleaning door.

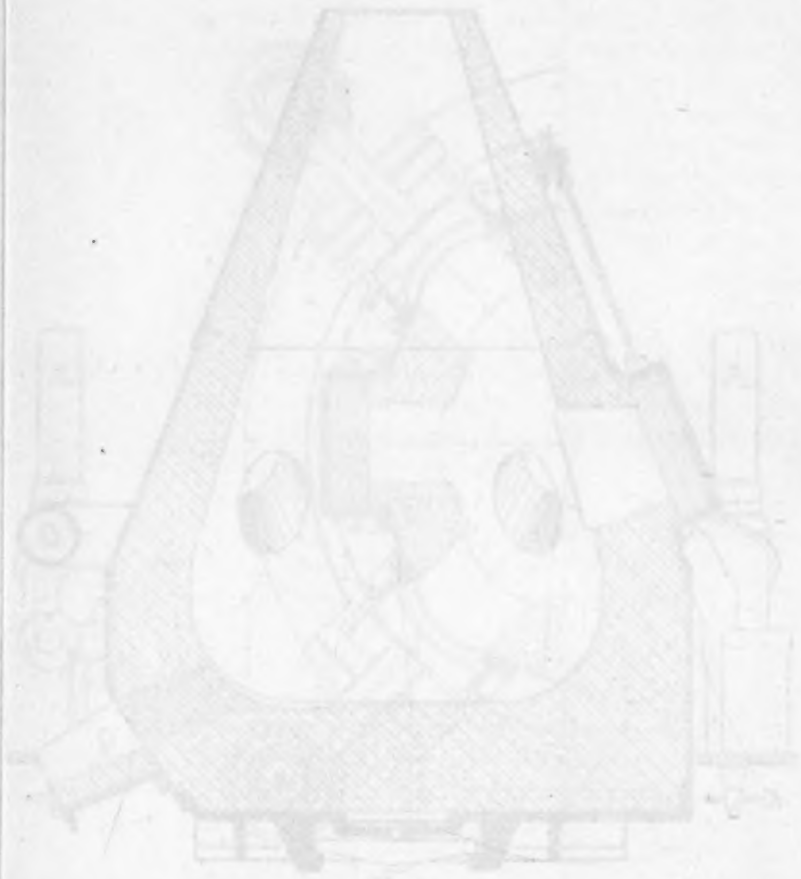


Details of the Gordon Converter.





Details of the Gordon Converter.



The operations are carried on as follows:

The exhaust valve to pipe *N* is left open at first and the blast admitted through the main *O*. There being no pressure in the cylinder on top of the piston, the latter remains up against the head. The valve *H* at this moment is seated, so that the blast, which enters through the slots *F*, is not wasted, the only escape being through the holes *a*, *a*. The metal is poured through the charging door *P*, the exhaust to pipe *N* is then closed, so that the blast passes through the holes *b*, *b*, drilled in the piston from which the tuyeres depend, and the latter assisted by gravity and pressure of blast upon the unbalanced piston *E*, descends into the metal. The valve *H*, stopped at the proper point by the nut *L*, unseats, and the total pressure reaches the bath through the tuyeres. The holes *a*, *a*, on each side of the valve seat *I*, admit air enough to keep the tuyeres clear of the metal, until the valve *H* is quite opened.

When the blow is over, the exhaust to pipe *N* is opened, so that the pressure of blast is exerted only on the under side of the piston, which causes it to ascend, and at the same time remove the tuyeres from the bath. As there is but one common exhaust worked from the pulpit, all the tuyeres are withdrawn simultaneously, the metal may then be tapped.

To replace the tuyeres, the piston is kept against the head of the cylinder, and the apparatus is tilted into the position, shown by the dotted lines.

The stem of the valve *H* is used to force the piston upwards, so that the tuyeres stand out and are easily got at.

The piston is prevented from revolving by stops on its under side (not shown in drawings) and, by seizing the sleeve *D* with a pair of tongs, the tuyere holder *C* may be disengaged, and a new set placed in. This operation is performed quickly and does not necessitate the stopping of the engine.

The piston *E* is scored all around, so that sufficient blast may pass out to keep the tuyere holes, in the side of the converter, free from slag.

From this description, it will be seen that the mechanism used in moving the tuyeres is entirely outside of the converter, and being preserved by the cool blast, is not liable to destruction by the heat.

It will also be seen that, although all the motions are automatic, the construction remains very simple and allows of an efficient control of all the parts at any time during the operations.

The fourth and last process, based on the Bessemer principle, which I shall describe briefly, is the Avesta process.

The Avesta process is really a scientifically constructed and conducted Bessemer process, on a small scale.

*The converter is suspended on trunnions like the ordinary Bessemer converter, but instead of the expensive hydraulic machinery used in turning, it is turned by one man by means of a crank.

The converter in use is only 1·4 metres high and one metre in diameter. The holes from the tuyeres cover an area equal to that of a circle 200 millimetres in diameter. There are ninety holes, about three to three and one-half millimetres in diameter, and the area is so calculated that after the blast has passed through the metal, there is little or no free oxygen left in the escaping gases.

The charge weighs from 170 to 765 kilograms, and only eight-tenths per cent. of seventy per cent. ferro-manganese is used to recarbonize.

The pressure of blast is 1·04 kilos to the square centimetre.

The bottom is forced into place by a screw arrangement and can readily be detached and replaced.

I give below two analyses and physical tests of the metal produced by this process:

C.	*Si.*	*Mm.*	*P.*	*S.*	*T.S.*	*E.*
20	·05	·31	·051	—	35 K per square mm.	25% in 200 mm.
25	·11	·34	·05	—	37	30

Slag tests by Eggertz show from ·05 to ·5 per cent.

It is interesting and instructive to know that Mr. W. F. Durfee, of Bridgeport, Conn., anticipated the first three of the above processes by about twenty-two years, and actually built, in 1864, a converter exactly similar in principle, and differing but little in detail from those described.

If he had had the means or opportunity of continuing his

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*Oesterreichische Zeitschrift. 1884. By Professor Josef von Ehrenwerth. Translated by J. P. L. Westesson.

experiments, he would undoubtedly have produced a product equally good as that of the Clapp-Griffiths, or other similar processes. *Plate XIII* shows a sectional elevation of the Durfee stationary converter.

In concluding my remarks, I would state that I consider it absolutely certain, that one of the above methods will very shortly supersede puddling.

Enough money has been invested to render it certain, that all the difficulties which invariably arise in attempting something

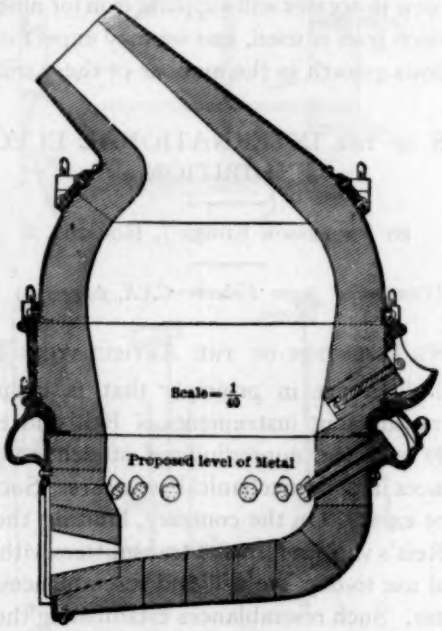


PLATE XIII.—The Durfee Stationary Converter.

new, will be solved. There can be no more steps backward and the puddling furnace is, therefore, inevitably doomed.

I believe, however, that in practice it will be found that there are two or three very important difficulties that will have to be overcome before steel entirely supersedes iron.

It is well known that a tolerably *good* bar iron can be made from high phosphorus pig, while the same pig converted into Bessemer steel is utterly worthless. The reason for this must be discovered and the cause removed.

Again, iron is fibrous (at least to some extent) even when made from the commonest stock, and although it will not show much elongation, will bend sooner than break, whereas steel made from the same stock is apt to be as brittle as glass, a sharp blow of a hammer often sufficing to crack the finished bar or plate. If some one will only discover how to make steel fibrous, the problem will be solved at once. Perhaps the Avesta process, by introducing a little slag into the steel, accomplishes this object.

There is no question, however, but that the material already made by these new processes will supplant iron for nine-tenths of the purposes for which iron is used, and we may expect in the next few years a marvellous growth in the number of these small converters.

GLIMPSES OF THE INTERNATIONAL ELECTRICAL EXHIBITION.

By Professor Edwin J. Houston.

(*Continued from Volume CXX, page 64.*)

No. 8.—Reis's Invention of the Articulating Telephone.

The radical difference in principle that is claimed to exist between the transmitting instruments of Bell and those of Reis would naturally lead the unprejudiced student to expect to find marked differences in their mechanical structure. Such differences, however, do not exist. On the contrary, limiting the inquiry to a comparison of Reis's various forms of transmitters with those in general commercial use to-day, we will find resemblances of the most marked character. Such resemblances establishing the fact we have already insisted on elsewhere, that Bell's contribution to the articulating telephone consisted in the production of a modification of an existing type of apparatus, and not the invention of a new genus.

Since the magneto-electric transmitter, such as described by Bell in his first patent, is in but comparatively limited use, we will exclude such apparatus from the present comparison.

Directing this comparison first to the mechanical structure, and second to the manner of operation of the various transmitters, it will be noticed that all transmitters of the type under consideration consist, so far as their mechanical structure is concerned, of three distinct parts, viz.:

(1.) The speaking tube, or mouth piece.
(2.) The diaphragm, or membrane.
(3.) The contact pieces, or apparatus for varying the contact.

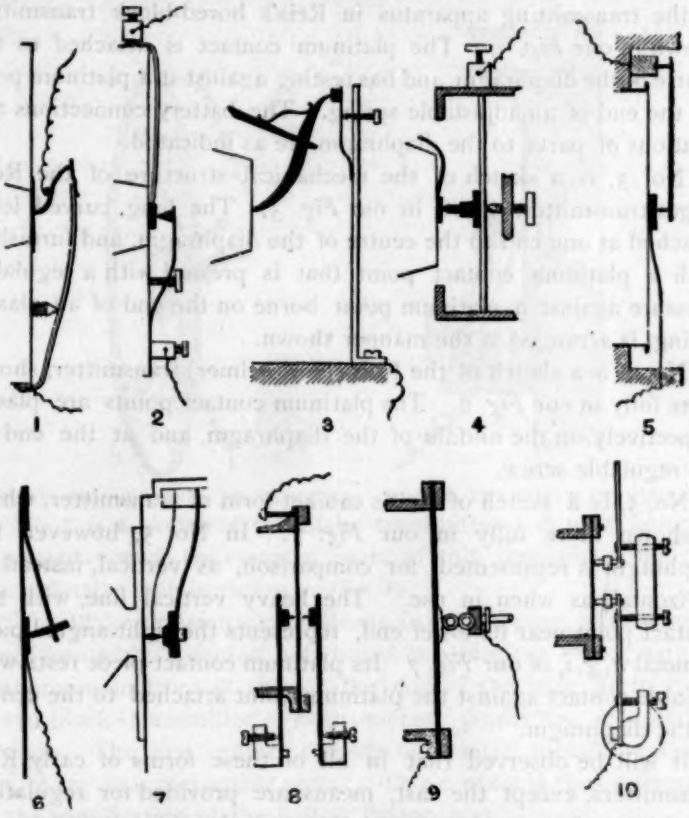


FIG. 10.—COMPARISON OF REIS AND MODERN TRANSMITTERS.

In *Fig. 10*, which in common with *Figs. 11, 12, 13 and 14* is taken from the work of Prof. S. P. Thompson on the "Telephone," only necessary parts are represented. The relative positions of the contacts and the diaphragm are, however, clearly shown in all.

In the upper row of sketches, from No. 1, to No. 5, inclusive, are shown various forms of transmitters, designed and constructed by Reis.

No. 1, is a sketch of Reis's original human-ear transmitter, shown in our previous article, in *Fig. 1*. The platinum tipped end of the bent lever and its loose contact with the platinum point of the

spring, together with their relations to the membranous diaphragm, are clearly shown.

No. 2, is a sketch showing the details of mechanical structure of the transmitting apparatus in Reis's bored-block transmitter, shown in our *Fig. 3*. The platinum contact is attached to the centre of the diaphragm, and has resting against it a platinum point on the end of an adjustable spring. The battery connections and relations of parts to the diaphragm are as indicated.

No. 3, is a sketch of the mechanical structure of the Reis-Legat transmitter, shown in our *Fig. 5*. The long, curved lever attached at one end to the centre of the diaphragm, and furnished with a platinum contact point that is pressed with a regulable pressure against a platinum point borne on the end of an elastic spring, is arranged in the manner shown.

No. 4, is a sketch of the Reis (Horkheimer) transmitter, shown more fully in our *Fig. 6*. The platinum contact points are placed respectively on the middle of the diaphragm, and at the end of the regulable screw.

No. 5, is a sketch of Reis's cabinet form of transmitter, which is shown more fully in our *Fig. 7*. In No. 5, however, the diaphragm is represented, for comparison, as vertical, instead of horizontal, as when in use. The heavy vertical line, with the contact point near its lower end, represents the right-angled piece of metal *h, g, i*, of our *Fig. 7* Its platinum contact piece rests with variable contact against the platinum point attached to the centre of the diaphragm.

It will be observed that in all of these forms of early Reis transmitters, except the last, means are provided for regulating the degree of contact or pressure between the contact pieces. In the last form, as will be readily understood, no such regulating device is required, since such adjustment is secured by the weight and inertia of the movable or upper contact piece.

No. 6, is a sketch of Berliner's transmitter. As we have not yet described these forms of transmitters, we will give more detailed drawings of the same. In *Fig. 11*, is shown the essential parts of the Berliner transmitter.

The contact piece *E*, is securely attached to the centre of the diaphragm. A second contact piece, *k*, of the form shown, is hung from the arm *I*, which is loosely jointed at *N*, so as to per-

mit the contact *k*, to be pressed against *E*, by the weight of the swinging contact and its supporting arm. The electrical connections are as shown in the figure.

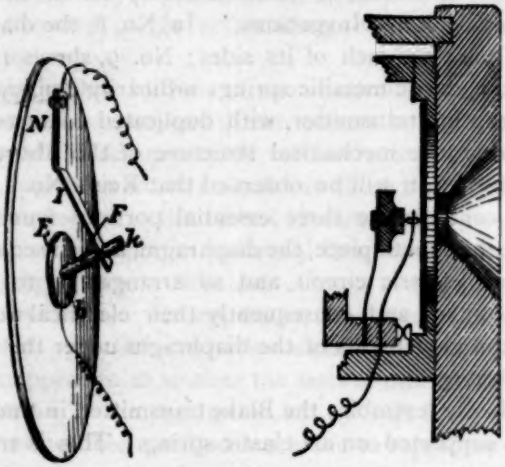


FIG. 11. THE BERLINER TRANSMITTER. FIG. 12. THE BLAKE TRANSMITTER.

No. 7, is a sketch of the Blake transmitter. A more complete representation of the essential parts of this transmitter, omitting the induction coil, is shown in *Fig. 12*.

The Blake transmitter, as shown in the drawing, has its mouth piece formed by a conical hole bored in a block of wood, with the diaphragm in front of the smaller end of the hole, as in Reis's bored-block transmitter. Both contact points are mounted on springs. The first contact consists of a small metallic spike, or pin, of any non-corrodable metal. This contact is thus referred to in the specification of the British Patent, viz:

"It is desirable that it should be formed of, or plated with, some metal like platinum or nickel, which is not easily corroded. It may be attached directly to the diaphragm, but I prefer to support it independently, as shown, upon a light spring.

The other electrode is attached to a comparatively heavy weight, also supported by a spring, as shown. Of this electrode, the specification states: "This weight may be of metal, which may serve directly as the electrode, but I have obtained better results by applying to it, at the point of contact with the other electrode, a piece of gas-coke, or a hard-pressed block of carbon."

These contacts, with their supporting springs, and their posi-

tions with respect to the diaphragm and mouth piece, are show in sketch No. 7.

Nos. 8, 9, and 10, are various forms of transmitters devised by Edison, and described, or referred to by us in the article on "Edison's Telephonic Inventions." In No. 8, the diaphragm has a contact point on each of its sides; No. 9, shows rubber hose replacing the elastic metallic springs ordinarily employed; No. 10, is an interrupting transmitter, with duplicated contacts.

Comparing the mechanical structure of the above apparatus with one another, it will be observed that Reis's No. 1, or original apparatus, contains the three essential portions found in all the others, viz., the mouth piece, the diaphragm, and the contact points, placed in an electric circuit, and so arranged as to cause their degree of contact, and consequently their electrical resistance, to vary with the movements of the diaphragm under the influence of the sound-waves.

No. 2, closely resembles the Blake transmitter, in that one of the contacts is supported on an elastic spring. This is true of all of Reis's transmitters.

No. 4, still more closely resembles the Blake transmitter, as will be seen by the comparison of similar parts in each.

No. 5, resembles both the Beliner and the Blake transmitters, in the fact that the weight and inertia of the swinging contact are utilized for producing variations in the degree of contact on the movements of the diaphragm.

The resemblance of Nos. 8, 9, and 10, with the various forms of Reis transmitters, will be evident on comparing similar parts.

In all the later forms of transmitters shown, from No. 6, to No. 10, inclusive, the three essential parts of the instrument, viz., the mouth piece, the diaphragm, and the contact points are not only present, but also sustain identically the same relations to one another as they do in the original Reis apparatus. Indeed, as is evident, they are in most cases almost exact imitations of Reis's apparatus. As far as their mechanical structure then is concerned, a careful comparison fails to reveal any difference between the original apparatus which Reis constructed for the purpose of transmitting articulate speech, and the modified apparatus of later days. And yet it is claimed that the former apparatus are not true articulating telephones while the latter are.

Since the transmitting telephone operates mechanically on the

electrical circuit, and transmits articulate speech solely in virtue of such mechanical action, if no mechanical differences in the structure of Reis's forms of apparatus can be pointed out by the advocates of Bell, then we submit that they must in fairness recognize Reis as the true inventor of the articulating telephone, and Bell as the modifier only.

The description left by Reis of the construction of his various forms of transmitters is, beyond doubt, correct in all its details, and is such as would enable any skilled mechanic to go to work now and produce apparatus entirely similar to those described.

Let us now proceed to the second branch of the enquiry, viz., to the method of operation of the early Reis transmitters.

After having given a full and correct description of the mechanical structure of his apparatus, Reis proceeds to explain the method of its operation. Whether such explanation is correct or not, does not appear to us to alter the facts of mechanical structure of the apparatus. If the description left by Reis is such a full and exact one as will enable a skilled mechanic to reproduce the apparatus, and if such reproduced apparatus, without any modification or change whatever, is capable of transmitting intelligible, articulate speech, then Reis invented the articulating telephone, and publicly disclosed his invention in such a full, clear and exact manner as to enable others to make and use the same, and he therefore must be awarded priority of invention over Bell.

Of course, in the preceding it is understood that along with a full description of the mechanical structure of the apparatus, the inventor has added an explanation of the manner in which it was to be used. It was, of course, to be clearly stated that the transmitter was to transmit speech, and the receiver was to receive it. Such a description is not wanting. The person is to talk or sing against the transmitting membrane, or into the speaking tube, or its vicinity, and the listener is to place his ear near or close to the receiving diaphragm. Nothing else is necessary. Let, us therefore, look into this in connection with Reis's explanation of the manner of operation of the different parts.

That explanations as to the method of using the apparatus are not wanting, the following quotation will show, for example, in Reis's article, entitled "On Telephony by the Galvanic Current," published in the *Jahresbericht*, of the Physical Society of Frankfurt-am-Main for 1860–61, we find the following, viz.:

"If now tones, or combinations of tones, are produced in the neighborhood of the cube, so that waves of sufficient strength enter the opening *a*, they will set the membrane *b*, in vibration. At the first condensation, the hammer-shaped little wire *d*, will be pushed back. At the succeeding rarefaction, it cannot follow the return vibration of the membrane, and the current, going through the little strip (of platinum), remains interrupted so long as until the membrane, driven by a new condensation, presses the little strip (coming from *p*) against *d*, once more. In this way each sound-wave effects an opening and a closing of the current.

"But, at every closing of the circuit, the atoms of the iron needle, lying in the distant spiral, are pushed asunder from one another. (Müller–Pouillet, *Lehrbuch der Physik*, see page 304, of vol. ii., fifth edition.) At the interruption of the current, the atoms again attempt to regain their position of equilibrium. If this happens then in consequence of the action and reaction of elasticity and traction, they make a certain number of vibrations, and yield the longitudinal tone of the needle. It happens thus when the interruptions and restorations of the current are effected relatively slowly. But if these actions follow one another more rapidly than the oscillations due to the elasticity of the iron core, then the atoms cannot travel their entire paths. The paths travelled over become shorter the more rapidly the interruptions occur, and in proportion to their frequency. The iron needle emits no longer its longitudinal tone, but a tone whose pitch corresponds to the number of interruptions (in a given time). But this is saying nothing less than that the needle reproduces the tone, which was imparted to the interrupting apparatus."

Omitting the explanations as to the theory of its operations, the above clearly states the manner in which Reis intended his apparatus to be used. The receiving instrument being placed in the circuit of the transmitter and a voltaic battery, as elsewhere directed, one talks or sings into the mouth piece of the transmitting apparatus, while another listens at the receiving apparatus. All this precisely as in the case of the so-called modern invention.

So, too, in a letter written in English by Reis, in July, 1863, to Mr. Ladd, of London.

* * * * * * * * * *

"Tunes and sounds of any kind are only brought to our concep-

tion by the condensations and rarefactions of air or any other medium in which we may find ourselves. By every condensation, the tympanum of our ear is pressed inwards; by every rarefaction, it is pressed outward, and thus the tympanum performs oscillations like a pendulum. The smaller or greater number of the oscillations made in a second gives us, by help of the small bones in our ear and the auditory nerve, the idea of a higher or lower tone."

* * * * * * * * * *

"The apparatus consists of two separated parts: one for the singing station *A*, and the other for the hearing station *B*." See *Fig. 12*.

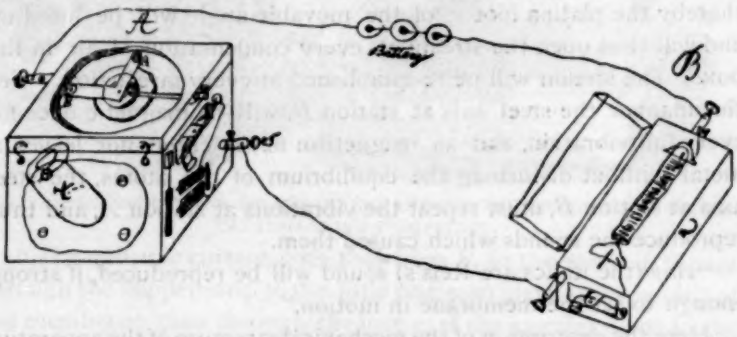


FIG. 13.—PEN AND INK SKETCH OF TELEPHONE ACCOMPANYING REIS'S LETTER TO LADD.

"The apparatus *A*, a square box of wood, the cover of which shows the membrane *c*, on the outside, under glass. In the middle of the latter is fixed a small platina plate, to which a flattened copper wire is soldered on purpose to conduct the galvanic current. Within the circle you will further remark two screws. One of them is terminated by a little pit in which you put a little drop of quicksilver; the other is pointed. The angle, which you find lying on the membrane, is to be placed according to the letters with the little whole (hole) *a*, on the point *a*, the little platina foot *b*, into the quicksilver screw, the other platina foot will then come on the platina plate in the middle of the membrane.

"The galvanic current coming from the battery (which I compose generally of three or four good elements) is introduced at the conducting screw near *b*, wherefrom it proceeds to the quicksilver, the movable angle, the platina plate and the complementary telegraph to the conducting screw *s*. From here, it goes through the conductor to the other station *B*, and from there it returns to the battery.

"The apparatus *B*, a sonorous box, on the cover of which is placed the wire-spiral with the steel axis, which will be magnetic when the current goes through the spiral. A second little box is fixed on the first one, and laid down on the steel axis to increase the intensity of the reproduced sounds. On the small side of the lower box, you will find the correspondent part of the complementary telegraph. [The device here alluded to was a small electro-magnetic signaling device, corresponding to the call-bell.]

"If a person sing at the station *A*, in the tube *x*, the vibrations of air will pass into the box and move the membrane above; thereby the platina foot *c*, of the movable angle will be lifted up and will thus open the stream at every condensation of air in the box. The stream will be re-established at every rarefaction. For this manner the steel axis at station *B*, will be magnetic once for every full vibration, and as magnetism never enters nor leaves a metal without disturbing the equilibrium of the atoms, the steel axis at station *B*, must repeat the vibrations at station *A*, and thus reproduce the sounds which caused them.

"*Any* (the italics are Reis's) sound will be reproduced, if strong enough to set the membrane in motion."

Here the description of the mechanical structure of the apparatus, the disposition of its various parts, and the manner in which they are intended to be used, are far more explicit than in many patent specifications, and in particular more explicit than the directions to be found in the specification of Bell's so-called telephone patent of 1876.

Besides the above, we quote Prof. Thompson's translation of a prospectus that was issued with the Reis apparatus, sold by Albert of Frankfort.

TELEPHON.

"Each apparatus consists, as is seen from the above illustration, of two parts: the telephone proper, *A*, (*Fig. 14*) and the reproduction apparatus (Receiver) *C*. These two parts are placed at such a distance from each other, that singing, or the tones of a musical instrument, can be heard from one station to the other in no way except through the apparatus itself.

"Both parts are connected with each other, and with the battery *B*, like ordinary telegraphs. The battery must be capable of effecting the attraction of the armature of the electro-magnet

placed at the side of station *A*, (3—4 six-inch Bunsen's elements suffice for several hundred feet distance).

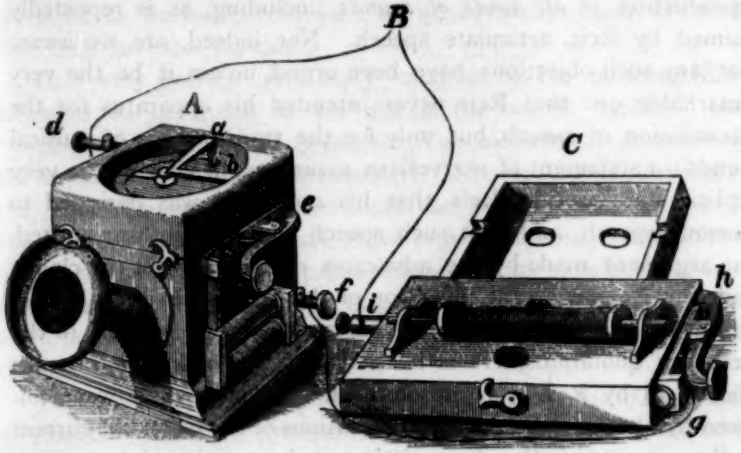


FIG. 14.—ILLUSTRATIONS OF APPARATUS AND CIRCUIT CONNECTIONS ACCOMPANYING REIS'S PROSPECTUS.

"The galvanic current goes then from *B*, to the screw *d*, thence through the copper strip to the little platinum plate at the middle of the membrane, then through the foot *c*, of the angular piece to the screw *b*, in whose little concavity a drop of quicksilver is put. From here, the current then goes through the little telegraph apparatus *e*, *f*, then to the key of station *C*, and through the spiral past *i*, back to *B*.

"If now-sufficiently strong tones are produced before the sound-aperture *S*, the membrane and the angle-shaped little hammer lying upon it are set in motion by the vibrations; the circuit will be once opened and again closed for each full vibration, and thereby there will be produced in the iron wire of the spiral at station *C*, the same number of vibrations which there are perceived as a tone or combination of tones (chord). By imposing the little upper case firmly upon the axis of the spiral, the tones at *C*, are greatly strengthened.

"Besides the human voice (according to my experience) there also can be reproduced the tones of good organ pipes from *F*—*c*, and those of a piano."

A careful consideration of the preceding quotations will render it clear that no objections can be found with the descriptions that

Reis gives of the mechanical structure of his apparatus, nor of the manner in which it is to be used in the transmission and reproduction of *all kinds of sounds*, including, as is repeatedly claimed by Reis, articulate speech. Nor, indeed, are we aware that any such objections have been urged, unless it be the very remarkable one that Reis never intended his apparatus for the transmission of speech, but only for the transmission of musical sounds—a statement of marvellous assurance, in view of the very explicit statement of Reis that his apparatus was designed to transmit speech, and that such speech was actually transmitted. The argument made by the advocates of Bell against the claims made for Reis as to the invention of the telephone, refers almost entirely to the theory of its actions as advanced by Reis in the preceding quotations. It is insisted that the makes and breaks referred to by Reis were absolute and complete, and were followed by absolute and complete cessations of the electrical current in the circuit of the transmitting and receiving instruments. That such currents cannot by any possibility act to reproduce articulate speech, and since, as they claim, Reis intended to describe such makes and breaks, his apparatus never could have transmitted or reproduced such speech.

It never seems to have occurred to such objectors that if even the smallest possibility of misunderstanding existed as to Reis's interpretation of the words "make" and "break," a spirit of but ordinary fairness would have led to the inquiry as to whether the original Reis apparatus would transmit articulate speech as he and others claimed, in which case it would appear that either,

(1) The assumption made by them that makes and breaks, complete and absolute, cannot transmit articulate speech is untrue; or,

(2) That Reis did not intend to convey the same meaning by his use of the words makes and breaks, as they assumed him to do, but that the makes and breaks were the variations in the electrical resistance of the circuit, that exactly corresponded to the variations in the movements of the transmitting diaphragm when acted on by the impinging sound-waves, or, as Reiss says in the *Jahresbericht* article, "to set up vibrations whose curves are like those of any given tone or combination of tones;" or,

(3) That, although the description given by Reiss of the manner in which his apparatus actually operated, was inaccurate, yet

the mechanical structure of the apparatus, the relative proportions of its parts, and the method in accordance with which they were intended to be used, were such as would enable them to do all that their inventor claimed.

That very grave doubts fairly exist as to the meaning Reis intended to be given to his use of the words "make" and "break," there can be no question. The mechanical structure of the apparatus itself, and the masterly manner in which Reis, in the *Jahresbericht* article, considers the nature of the curves in the case of compound sounds, prove beyond reasonable doubt that he clearly understood the principles according to which the telephone transmitter varied the intensity of an electrical current, so as to permit it to reproduce in the receiver the sounds made at the transmitter.

This matter will, perhaps, be made clearer by an examination of the Reis and other transmitters, shown in *Fig. 10*, while discussing the method in which they act while in actual operation.

Assuming, for the sake of argument, the correctness of the explanations generally given as to the manner of operation of, say, any of Edison's contact transmitters, the explanation would be as follows, viz.: The sound-waves impinging on the diaphragm impart to it vibrations similar, in their movements and complexity, to the vibrations they would also produce in the ear of a person standing near the speaker. The movements so produced in the diaphragm vary the pressure of the contact points and cause corresponding variations in the electrical resistance of the circuit in which the variable-resistance contact points are placed. These variations acting on the magnetic receiver at the receiving end of the circuit, reproduce in its diaphragm the motions of the transmitting diaphragm, so that one listening at the receiving instrument, hears whatever sounds are made at the transmitting instrument.

In order to avoid the too great movements of the contact points, by means of which one of them would be thrown completely off from the other, *and thus be prevented from receiving the impress of the movements of the diaphragm while it was separated from it*, one or both of the contact points are mounted on springs, the pressure of which against the other point on the diaphragm is controlled by regulable screws. When properly adjusted, the

contact point follows the motions of the diaphragm, and instead of actually leaving it completely and entirely, only leaves it at certain points of its surface, thus varying the cross section of the conducting surfaces in contact, and, of necessity, their electrical resistance.

If it were a matter of indifference whether the contact point occasionally left the diaphragm long enough to prevent it from receiving the impress of all its to-and-fro movements, the regulating screw would be unnecessary. The presence of this screw when taken in connection with the elastic support of one of the contacts, would appear clearly to show that the inventor intended, when he produced such a mechanical structure, to avoid the too free movement of the contact. That he intended the contact to be influenced by all the movements of the diaphragm; that he desired especially to avoid such a free movement of the contact point as would prevent it from leaving the diaphragm for such a length of time as to cause it to miss some of the to-and-fro movements of the diaphragm.

In the Berliner transmitter, the weight and inertia of the swinging contact permits the suppression of the elastically-mounted contact and its adjusting screw. In the Blake transmitter the elastic support is given to both contacts, their weight and inertia permitting the suppression of the adjusting screw.

Applying the preceding description of the mode of operation of Edison's, Berliner's and Blake's transmitters to sketches 1, 2, 3, 4, and 5, of Reis's transmitters, we fail, after careful thought to perceive any differences in their mode of operation. Their mechanical structure and the relative arrangement and proportion of their parts are practically the same. The action of the sound-waves on the diaphragm and its action on the contact points are the same, so that, whatever one does, the other does; whatever one is, the other is.

Now, in considering the only fair and proper meaning to attach to Reis's use of the words "make" and "break," we would call attention to the following important points; viz., that where necessary to prevent such makes and breaks as would be injurious to proper action, we find in all of Reis's transmitters:

(1.) Elastically-supported contact points.

(2.) Regulable screws provided for varying as desired the pressure of the contact point against the diaphragm, and that,

moreover, when such devices are unnecessary, as in No. 5, they are markedly absent.

In view, then, of these facts, clearly shown by the mechanical structure of his apparatus, we find it difficult to understand how any one is willing to maintain that Reis intended that the "makes" and "breaks" he alludes to in his description of the theory of operation of his apparatus, were of such a nature as to produce such a free movement of the variable contact point, *as would throw it completely off or away from the diaphragm, so as to cause it to lose some of its to and fro movements.* That such was never intended, is not only proved by the elastically supported contact and its regulating screw, but also by the direct statements of the inventor. He speaks thus of the contact, "the little hammer-shaped wire *d*, being pushed back," at each condensation, which would be clearly impossible, if it lost some of the condensations. He asserts that "the movable angle (of his cabinet apparatus) will be lifted up and will thus open the stream at every condensation of air in the box, the stream will be re-established at every rarefaction."

That Reis could not have intended his use of the words "make" and "break" to signify such a separation of the elastically-supported contact as would remove it from the diaphragm long enough to permit it to miss some of its vibrations, should need no further proof than a careful study of the very able manner in which he discourses, in his *Jahresbericht* article, the graphic representation of the alternate condensation and rarefaction of the air in sound-waves. His insight into this matter was remarkably clear, considering the time at which he wrote, and even the most advanced science of the present day has but little to add to what he has stated as to the representation, by means of graphic curves, of the variations in the density of the air produced by means of sound-waves. His writings show, in the clearest manner, that the prime condition he constantly kept before him, in order to solve the electrical transmission of sounds of all kinds, was the necessity of obtaining an exact reproduction, in the diaphragm of the receiving instrument, of the movements of the diaphragm of the transmitting instrument under the influence of the sound-waves. "The function of the organs of hearing, therefore, is to impart faithfully to the auditory nerve every condensation and rarefaction occurring in the surrounding medium." "That which is perceived by the auditory

nerve is, therefore, merely the action of a force affecting our consciousness, and as such may be represented graphically, according to its duration and magnitude, by a curve." "Our ear can perceive absolutely nothing more than is capable of being represented by similar curves, and this method is completely sufficient to bring before our clear consciousness every tone and every combination of tones." "As soon, therefore, as it shall be possible, at any place and in any prescribed manner, to set up vibrations whose curves are like those of any given tone *or combination of tones* (the italics are my own), we shall receive the same impression as that tone or combination of tones would have produced upon us."

"Taking my stand on the preceding principles, I have succeeded in constructing an apparatus by means of which I am in a position to reproduce the tones of divers instruments, yes, and even, to a certain degree, the human voice."

Can any reasonable doubt remain, after reading the above, that Reis could have intentionally designed his apparatus to produce effects contrary to what he so clearly stated as necessary for its success? This will, perhaps, be made still clearer from an inspection of the tables of curves accompanying his *Jahresbericht* article. These we will reproduce, as well as a translation of that part of the article alluding to them, even though we have quoted some of it previously. We will use for this purpose the translation introduced into the record of the "American Bell Telephone Company *vs.* Amos E. Dolbear, *et al.*"

"What our auditory nerve perceives is, then, simply the effect of a force coming within the range of consciousness, and this force can be represented, both as to duration and magnitude, graphically by a curve."

"Let *a*, *b*, (*Fig. 15*) represent any given time, and the curve above

FIG. 15.—GRAPHIC REPRESENTATION OF CONDENSATION AND RAREFACTION.

the line condensation (+), the curve below the line rarefaction (—), then any ordinate raised from the end of any abscissa will represent the degree of condensation, at the time represented by its base, in consequence of which the drum of the ear vibrates.

"Our ear can, under no circumstances, appreciate more than can be represented by these curves, and this, indeed, is entirely sufficient to give us a clear perception of any tone (*Ton*) or any combination of tones.

"If several tones (*Tonen*) are produced at the same time, the conducting medium is subjected to the influence of several simultaneous forces, and the two following laws will hold good. If the forces act in the same direction, the amplitude is proportional to the sum of the forces; if the forces act in opposite directions, the amplitudes are proportional to the difference of the opposing forces.

"If, for example, in the case of three tones, we draw the curve of condensation of each separately, then by a summation of the ordinates of corresponding abscissas, we can determine new ordinates and develop a new curve, which might be called the combination curve. This represents exactly what our ear perceives of the three simultaneous tones. The fact that the musician can distinguish the three tones need not surprise us any more than the fact that any one acquainted with the theory of colors can in green discover blue and yellow; but the combination curves in Plate I, (our *Plate III*,) show that this difficulty is a slight one, for in these curves all the relations of the components successively recur. In the case of chords, of more than three notes, the relations are not so readily seen from the drawing, Plate II, (*Plate IV*), for example. In the case of chords, however, the skilled musician also finds difficulty in recognizing separate notes.

"Plate III, (*Plate V*) illustrates discord (*Dissonanz*). Why discords impress us unpleasantly, I will leave my readers to judge at this time, though I may, perhaps, return to the subject subsequently in another paper.

"From the preceding, it follows:

"*First.*—Every tone and every combination of tones, on striking our ear, causes vibrations of the drum of the ear, the succession of which may be represented by a curve.

"*Second.*—The succession of these vibrations alone gives us a conception (*Sensation*) of the tone, and every alteration changes the conception (*Sensation*)."

The terms condensation and rarefaction as applied to sound-waves are entirely relative in their nature. A medium transmitting

a sound-wave is rarefied when its density is decreased, and condensed when its density is increased beyond what it originally was, the extent or ratio of the condensations or rarefactions, representing the amplitude of the sound-waves. Practically, then, the amplitude represents the amount of energy charged on the medium in order to mould it into waves. In the case of interfering waves, the amount and distribution of the energy in the resulting wave depends on the amount and distribution in the component waves. When the direction in which the energy acts is the same in, say, two waves, the amplitude of the resultant wave is the sum of the amplitudes of the two; if the direction is opposite, the amplitude of the resultant wave is the difference of the two. Two waves may completely obliterate each other, provided they simultaneously affect the same particles of the medium in opposite directions, with equal intensities, and for equal lengths of time; or, as it may be otherwise expressed, if the waves meet in opposite phases, are of equal amplitudes, and are of the same wave lengths.

Considering now the curve shown in the upper part of *Plate IV*, as resulting from the simultaneous action of the notes c, e', g, and c', it will be noticed that the condensation rises while the four notes are simultaneously condensing the air, but begins to fall when part of them are endeavoring to rarefy it, as is indicated, for example, between the third and the sixth abscissa, where a gradual fall is shown, while between the sixth and seventh abscissa, and for some distance beyond, the fall is more sudden. Now, although in a general sense, it is correct to regard the curve between the first and the $7\frac{1}{2}$th abscissa as indicating a condensation of the medium propagating the wave, yet it is equally correct to regard all that part of the curve between the third and the $7\frac{1}{2}$th abscissa as representing a rarefaction of the medium, superposed on the condensation, since a decrease of the condensation at any time is practically a rarefaction. Moreover, its effect is practically the same as that of a rarefaction since if the condensation moves the medium in one direction, the rarefaction moves it in the opposite direction.

Studying Reis's curves in this light, is it not clear that he never could have intended to use the words "make" and "break" in the sense ascribed to him, but that when he spoke of the interruption occurring at every condensation and rarefaction, he uses these terms in the sense above indicated. Does not the elastically-sup-

TAFEL I.

Umkehrungen eines Dreiklanges.

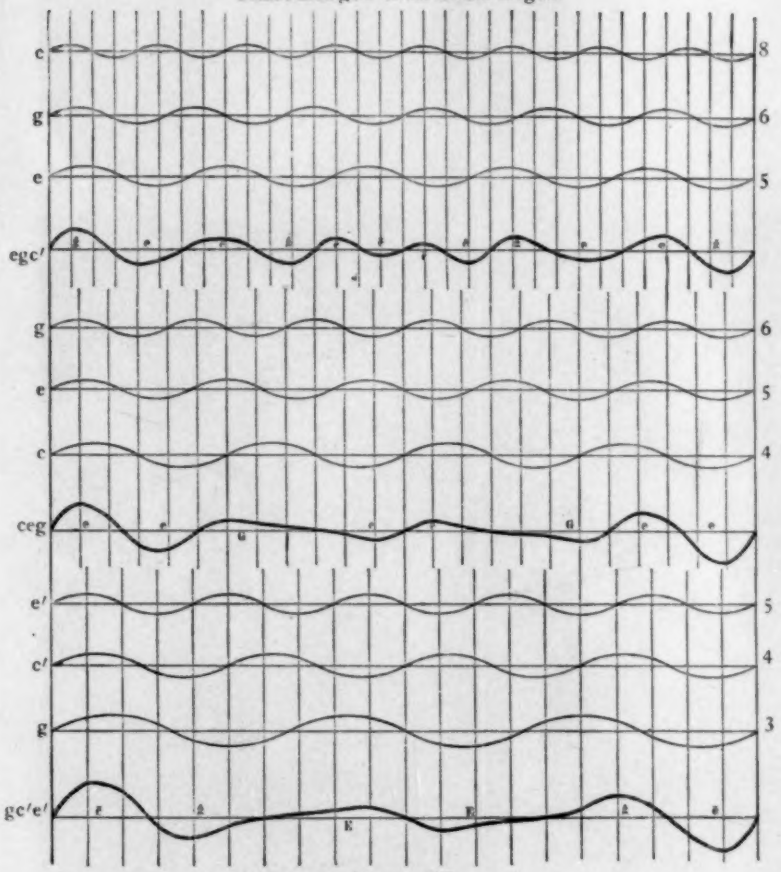


PLATE III.

TAFEL II.

C dur.

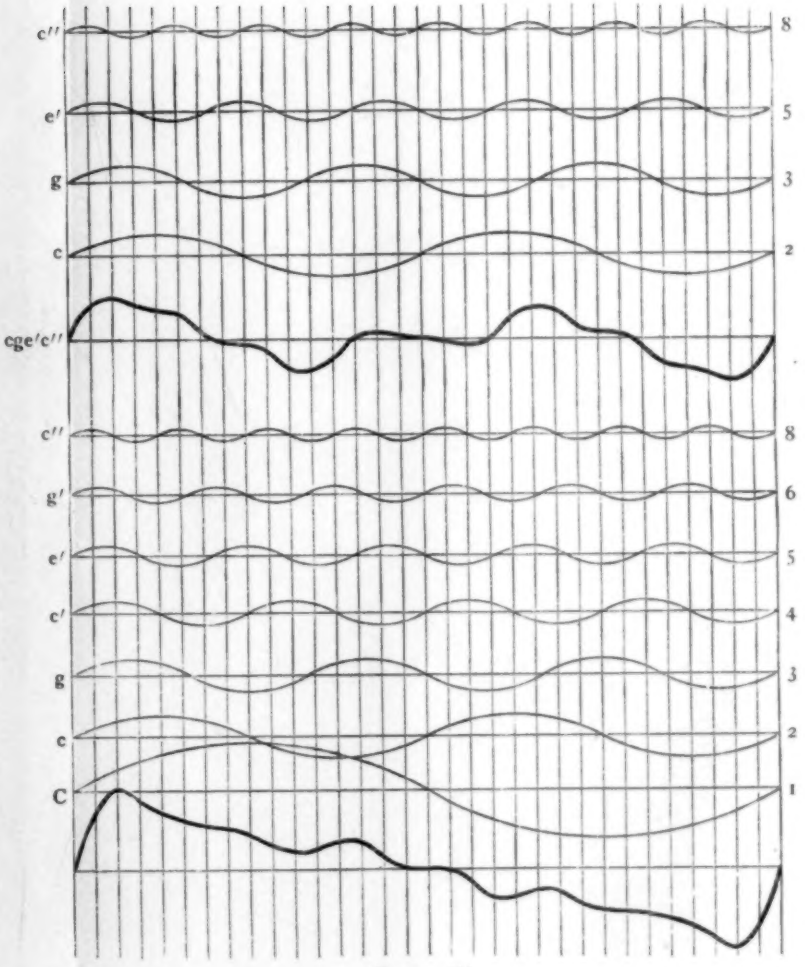


PLATE IV.

TAFEL II.

C dur.

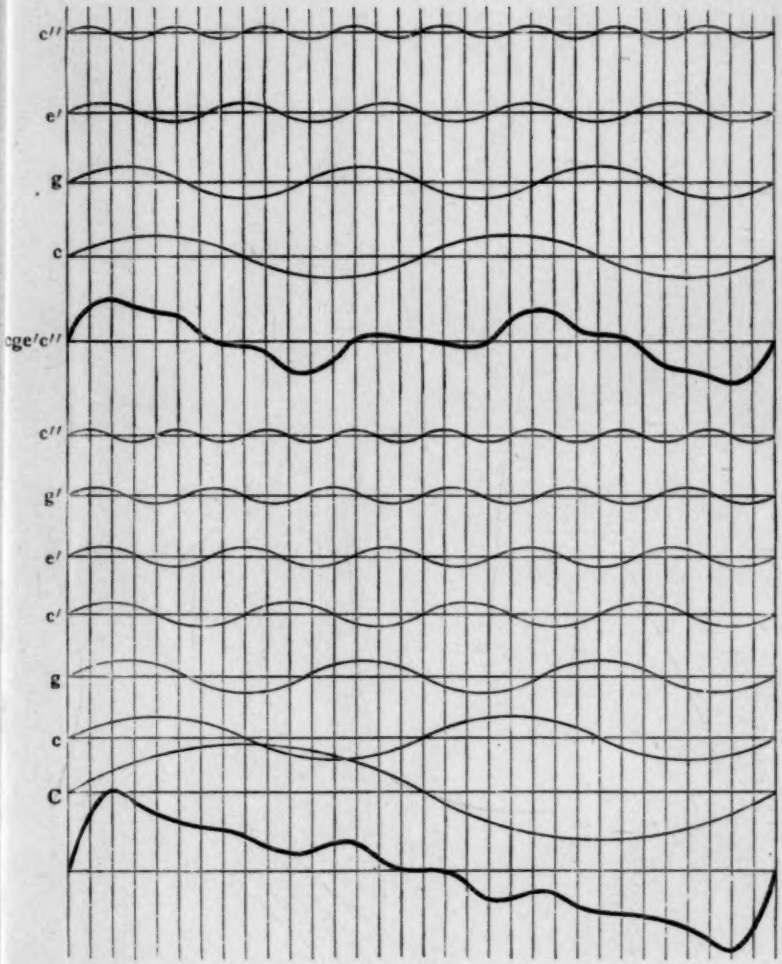


PLATE IV.

L II.

(*Houston.*)

TAFEL III.

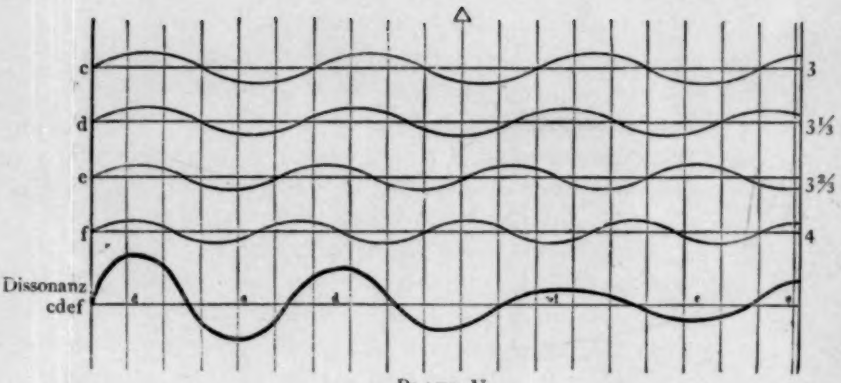
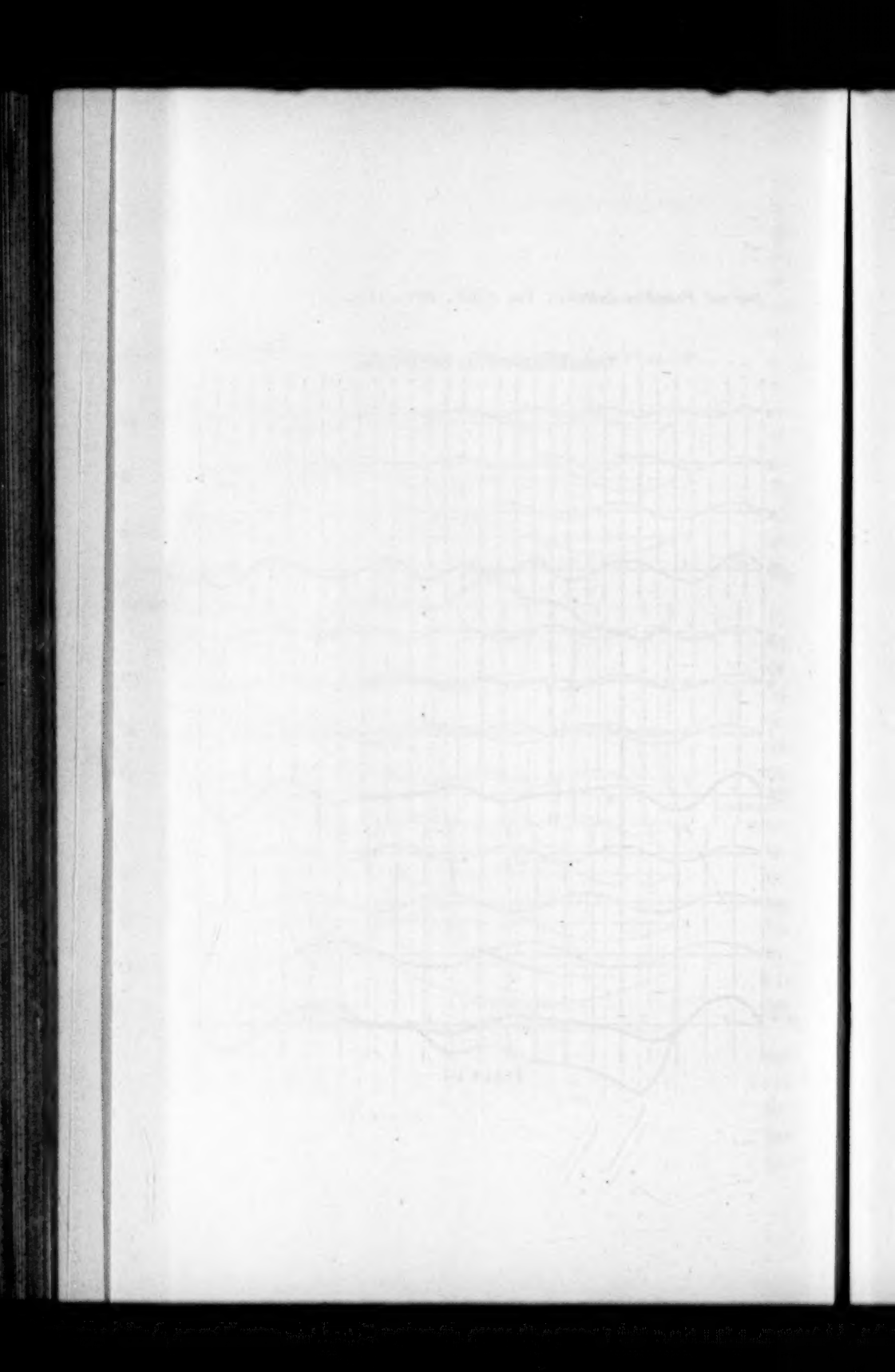


PLATE V.



ported contact, with its adjusting screw, show that he did not intend the contact to leave the diaphragm completely, but that he wished it to follow its finer motions? Does he not say in the *Jahresbericht* article, "but the quality of the note depends upon the number of variations of amplitude (*Anschwellungen*) occurring in the same time." "Now my apparatus gives the number of the vibrations, but with far less strength than the original ones: though also, as I have cause to think, always proportional to one another up to a certain degree. But because the vibrations are throughout smaller, the difference between large and small vibrations is much more difficult to recognize than in the original waves, and the wave is therefore more or less indefinite."

If his apparatus, as he left it, with the elastically-supported contact, and its adjustable spring, will speak in 1885, with absolutely nothing added to it, is it in the light of 1876, that it has been made to work? Is it not rather in the very clear light of 1860, and 1861, when the *Jahresbericht* article was published?

There are two other points in the *Jahresbericht* article which throw some light on the meaning, which should be given to Reis's use of the words "make" and "break." The first of these is to be found in his description of the action of the core of the magnetizing spiral. "The intensity also of this tone is proportional to that of the original one; for, in proportion as this is more intense, the motions of the membrane are greater, the motions of the hammer, also, and finally the time during which the circuit remains opened is greater, and, consequently, up to a certain limit the motions of the atoms in the reproducing wire are greater, we perceiving them as greater vibrations in just the same way as we would have perceived the original sound-wave." If the "opening" of the circuit here referred to means, as we have suggested, its gradual increase in resistance, while the diaphragm was under the influence of the rarefaction, or while it was moving in a certain direction, then we can understand how the instrument could reproduce variations in the intensity of the transmitted sounds; but, if it mean a complete break, then, since it is difficult to understand how the circuit, if completely broken, can be anything more than broken, it is equally difficult to see how the duration of the complete break could effect the intensity.

Again, Reis's claims as to the capabilities of his instruments

"now in reference to the capabilities of the telephone," * * * "experiments showed that the sounding wire was capable of reproducing complete chords of three tones of the piano;" that is to say, it was capable of transmitting quality which would be clearly impossible, if the instrument acted on the "make" and "break" principle in the sense claimed by the advocates of Bell.

If the paragraphs in Reis's article, in which he refers to the "making" and "breaking" of the circuit stood by themselves, then it might be admissible to put the construction on them before referred to, but taken in connection with the other declarations and explanations of Reis, we do not see that such interpretation stands at all.

If, however, such interpretation be sustained, then it simply amounts to this: viz., that Reis did not understand the theoretical operation of his instrument. But we fail to see how this can injure his position as an inventor. If he produced an instrument whose mechanical structure was such as would permit it to reproduce articulate speech, and properly instructed others how to make and use it, then he is the true inventor of the same, no matter how erroneous his ideas may have been as to its theoretical operation. If, as is undoubtedly the case, scientific opinion of the present day is still uncertain as to what is the exact theory of the action of the articulating telephone, then Reis may justly be excused if he erred in his description of the same. Should Bell's theory of the undulatory character of the transmitted current, in the sense in which he claims it, be fallacious, as is by no means improbable, then are his claims to the modified forms of apparatus that he has produced to be regarded as untenable?

In the Legat description of Reis's apparatus, already referred to, we note the following in speaking of the movable contact: "It is advisable to make the arm *c, e*, longer than the arm *e, d*, in order that the least motion at *c*, may operate with the greatest effect. It is also desirable that the lever itself may be made as light as possible, *that it may follow the movements of the diaphragm* (our italics). Any inaccuracy in the operation of the lever *c, d*, in this respect will produce false tones at the receiving station." The necessity for the contact to follow the motions of the diaphragm is precisely what we have insisted on. Legat, for some reason or other, does not appear to have obtained any valuable results with the instruments with which he experimented.

It would appear to be an assumption, bordering on impertinence, to deny to the man whose genius created the most marvellous instrument of modern science, a knowledge of the simplest principles underlying the operation of the instrument he has constructed. It is incredible that he, who modelled his transmitting instrument after the human ear, who explained so thoroughly by curves the peculiarities of the motion of its tympanum, and who wrote, "the function of the organs of hearing, therefore, is to impart faithfully to the auditory nerve every condensation and rarefaction occuring in the surrounding medium; the function of the auditory nerve is to bring to our consciousness the vibrations of matter resulting at the given time, both according to their number and magnitude," could have so blundered, as to desire to obtain results that he especially designed the mechanical structure of his apparatus to avoid.

Any form of variable-contact transmitter will cease to properly operate if spoken to in too loud a tone, since in such a case the movable contact will be separated from the diaphragm too long to permit it to be impressed by all its motions. By the use, however, of the regulating screw the pressure of the elastically-supported contact may be varied, so as to permit a fair use with a loud tone. This is true with the Reis transmitters, and equally true of the others.

We again assert, therefore, that the Reis transmitters operate, when transmitting articulate speech, in precisely the same way as the modern receivers, and presumably in the very way that Reis intended that they should operate, but that, however, this may be, even admitting that he so strangely erred in his explanation of the way in which he conceived it to operate, then, nevertheless, *if his description of the mechanical structure of his apparatus, and of the manner in which it was intended to be used, is such as to enable a skilled mechanic with no other knowledge of the art than that imparted to him by Reis's publications and drawings, to make and use the same, then Reis is the first, true and only inventor, and to him must be awarded priority of invention over all others.*

Since the object of the contact pieces is to vary the electrical resistance of the circuit in which they are placed, by altering the extent of surfaces in contact, they must, of course, have been formed of some electrical conducting substance. Since, if such substance were liable to oxidation, it might, apart from the motions of the

diaphragm, thus introduce a varying resistance into the circuit, the substance must not be readily oxidized. Reis employed for his contact pieces metallic platinum. Berliner, Blake, and, indeed, nearly all later inventors have replaced the platinum by hard carbon, since Hughes pointed out its superiority. This material, though perhaps better in degree, does not differ in the manner of its operation, as is indeed shown by the fact that the Berliner or the Blake transmitters will operate satisfactorily when their carbon contacts are replaced by metallic platinum, or that conversely the Reis transmitter will operate if furnished with carbon contacts.

In the early history of the Reis-Bell controversy, it was denied by the advocates of Bell that it was possible to make the Reis apparatus talk. When, however, it was proved by the most unquestioned testimony, and by actual demonstrations, that the Reis apparatus could and would talk, instead of fairly according to him the merit of the discovery, they adopted the specious plea that even if the apparatus would talk, it was only in the light of the experience of Bell that it could be made to do so; that is, that the success of actual use of the Reis apparatus was by the use of the knowledge gained since 1876. Such reasoning would appear too illogical to need refutation. Let us, however, present the same reasoning in another light.

An apparatus designed by Reis to transmit articulate speech, made, we will say, in 1861, is left unnoticed in that strange way in which the world has too often failed to accept the gifts of genius, if born out of time. It slumbers amid the dust of age and neglect, while its inventor, unable to convince the world of its great value, at last dies disappointed and heart-broken. Temporarily forgotten, it is re-invented by another and accepted by the world as the greatest discovery of modern time, and no honors are deemed too great for its supposed inventor. Again brought to light, the original apparatus is compared with the later alleged invention and found to resemble it in a remarkable manner. Doing absolutely nothing but to remove the accumulated dust of many years, it is placed in circuit with another receiver, or, indeed, even with its own receiver, and lo! it talks. Surprised at this success, the experimenter searches for Reis's description of such apparatus and finds that he and others claim to have talked through such apparatus long ago. What then should be his indignant surprise, as one

possessing that true scientific instinct of desiring to know the truth for the truth's sake, to be told that Reis must have falsified his record when he stated that such an apparatus would talk; that had it not been for Bell, the Reis apparatus would never have spoken. If it be a telephone in 1885, why was it not equally a telephone in 1861, especially when it was designed to talk, and when, if the most solemn statements are to be believed, it did talk? It is not, perhaps, saying too much to add that a defence of such a character is open to grave suspicions of insincerity.

And yet, what is the position in which the advocates of Bell insist on placing him, in order to enable them to continue to maintain his patent rights in the United States? Do they not assume for him a declaration that might not unfairly be worded as follows: "I have made a grand and great discovery of so novel a character that I demand, for its protection, the broadest and most general patent that can be issued. I have discovered that if I use the regulating screw placed by Reis in an apparatus improperly called by him a telephone, and obtain a certain pressure of an elastically-supported contact against a diaphragm, that the apparatus will do what Reis and others claim that it did many years ago, but which I do not believe they ever accomplished. My discovery, therefore, is of so novel a character that I claim in general the use of electricity in the transmission of articulate speech, and consider it a sufficient proof of the infringement of my patent rights if any one hereafter succeeds in actually transmitting speech electrically."

Is Mr. Bell willing to permit others to continue to make such claims for him? We can hardly believe that his known sense of fairness will permit him to continue to do so. The credit that is legitimately due him, and which all are willing to acknowledge, should suffice, without his permitting others to endeavor to appropriate for him that due to Reis.

It has been said, "a century of Reis would not produce a speaking telephone," because Reis did not give the correct explanation of the manner of operation of his instrument. We beg leave to differ entirely from this statement. The apparatus of Reis, so far as its mechanical structure and the way in which it was designed to be used are concerned, are entirely independent of any theoretical explanations of how such apparatus operated to produce the

results for which they were designed. According to such reasoning, a century of Franklin would never produce a lightning rod, since Franklin, although he gave a full and correct description of the construction of his lightning rod, and explained how it was to be placed on the building it was designed to protect, alleged that the manner in which it operated was by drawing a hypothetical electrical fluid from the surcharged cloud and quietly conveying it to the ground. At the present, we do not believe in the fluid theory of electricity; therefore, there never was a lightning rod, and no such instrument can act as a lightning rod until we know the exact manner in which it lowers the higher potential of the cloud to that of the earth. Is not such reasoning nonsensical, and could it not be enlarged indefinitely?

It is an interesting fact that Bell approached the invention of his form of articulating telephone from very nearly the same standpoint as Reis; that is, from a study of the curves resulting from the combination of different tones, with, however, this difference. Reis designed his curves to represent the variations in the amplitude of the sound-waves, or the variations in the movements of the diaphragm of the receiving instrument. Bell, on the contrary, employed his curves to represent electrical undulations. Quoting from the specification of his U. S. Patent, No. 174,465, of March 7, 1876:

"Electrical undulations, induced by the vibration of a body capable of inductive action, can be represented graphically, without error, by the same sinusoidal curve which expresses the vibration of the inducing body itself, and the effect of its vibration upon the air; for, as above stated, the rate of oscillation in the electrical current corresponds to the rate of vibration of the inducing body; that is, to the pitch of the sound produced. The intensity of the current varies with the amplitude of the vibration; that is, with the loudness of the sound; and the polarity of the current corresponds to the direction of the vibrating body; that is, to the condensations and rarefactions of air produced by the vibration. Hence, the sinusoidal curve *A*, or *B*, *Fig. 4* (our *Fig. 16*), represents, graphically, the electrical undulation, induced in a circuit by the vibration of a body, capable of inductive action."

The horizontal line *a*, *d*, *e*, *f*., etc., represents the zero of current. The elevation, *b*, *b*, *b*, etc., indicates impulses of positive

electricity. The depression, *c*, *c*, *c*, etc., show impulses of negative electricity. The vertical distance, *b d*, or *c f*, of any portion of the curve from the zero line expresses the intensity of the positive or negative impulse at the part observed, and the horizontal distance

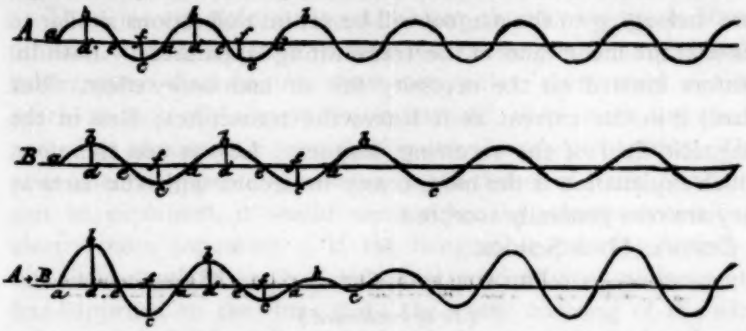


FIG. 16.—CURVES REPRESENTING ELECTRICAL UNDULATIONS FROM FIG. 4, OF BELL'S U. S. PATENT, NO. 174,465.

a, *a*, indicates the duration of the electrical oscillation. The vibrations represented by the sinusoidal curves, *B*, and *A*, *Fig. 4*, are in the ratio aforesaid, of 4 to 5; that is, four oscillations of *B*, are made in the same time as five oscillations of *A*.

"The combined effect of *A*, and *B*, when inducted simultaneously on the same circuit, is expressed by the curves $A + B$, *Fig. 4*, which is the algebraical sum of the sinusoidal curves of *A*, and *B*. This curve, $A + B$, also indicates the actual motion of the air when the musical notes considered are sounded simultaneously. Thus, where electrical undulations of different rates are simultaneously induced in the same circuit, an effect is produced analogous to that occasioned in the air by the vibration of the inducing bodies."

The difference between Reis and Bell, then, is as follows: Reis insisted on the necessity of reproducing in the diaphragm of the receiving instrument similar movements to those in the ransmitting diaphragm, and endeavored to obtain this result by an elastically-supported regulable spring, through the action of what he called makes and breaks. Bell also insisted on the reproduction in the receiving diaphragm of the motion of the transmitting diaphragm, but declared that in order to achieve this result exactly similar undulations must be given to the electrical current that traverses the wire between the two stations. Reis, it will be

observed, designed his curves to represent electrically the magnetization of the receiving electro-magnet, which, as Legat describes, "will be magnetized, and demagnetized correspondingly with the condensation and rarefaction of the mass of air," "and the armature belonging to the magnet will be set into vibrations similar to those of the membrane in the transmitting apparatus." Both inventors insisted on the necessity for an undulatory effect. Bell places it in the current as it leaves the transmitter; Reis in the magnetic field of the receiving magnet. Let us see, therefore, which explanation is the more nearly in accord with the facts as they are now generally accepted.

CENTRAL HIGH SCHOOL.

PHILADELPHIA, October 10, 1885.

(*To be continued.*)

INFLUENCE OF ELECTRIC STORMS ON SUBTERRANEAN TELEGRAPH WIRES.

TRANSLATED BY SAMUEL H. NEEDLES.

[*Proceedings of the Academy of Sciences, of Paris, June 22, 1885. Note of M. Blavier, Presented by M. Mascart.*]

Several years ago, when the construction of extensive subterranean telegraph lines commenced, now connecting the principal cities of France and Germany, it was expected that their conductors would be entirely protected from atmospheric storms. Each wire being covered with guttapercha, and a number of them united in a cable, they are further shielded by an outer covering of iron wire wound around the whole, or by a continuous cast-iron tube; and it is well-known that a body, if surrounded by a metallic envelope and the latter in communication with the earth, will remain neutral, whatever may be the outer electric condition.

It has, however, been found that occasionally on the occurrence of thunder storms, there are produced, at the stations served by the subterranean wires, discharges of electricity, which cause sparks and even melt the fine protecting earth-wires (*paratonnerres*). These accidents are much more rare, and are less injurious than is the case with aërial wires, and do not seem to be of such nature as to interfere with transmission. They are always co-incident with

thunder storms, commencing in the open country at considerable distance from cities, where, in the latter case, the subterranean telegraph wires are protected by the network of water and gas pipes, *below* which they are placed.

Thus, for example, during a violent thunder storm, on March 9th last, midway between Belfort and Besançon, France, on the subterranean line uniting these two cities, there were noticed sparks at each of the most distant terminii, while in the two places named an atmospheric perturbation was scarcely perceptible. This phenomenon, apparently contrary to the theory of static electricity, can be explained, it would seem, either by electro-dynamic or electro-static induction. If the telegraphic cable is buried at but little depth in earth of imperfect conductivity—as frequently has happened on the lines laid—the outer covering of the wires acquires, under the influence of storm clouds (and while the telegraph wires themselves remain in a neutral condition)—a greater or less charge of electricity. At the moment of a flash of lightning this charge becomes suddenly free—or at least partially so—and flows into the earth, *following the outer covering* in opposite directions. In the first place, there must be developed in the interior wires two induced currents in contrary directions, the difference between which alone would act upon the apparatus at the extreme stations. It appears, however, that the effect resulting therefrom must be very slight, the more so because the free current being rapidly lost in the earth induction must be very limited.

A second effect is, that the discharge of electricity contained in the outer covering of the cable not being instantaneous, its electric potential rapidly decreases for a moment, however brief that instant may be. The free current reacts on the interior wires, which suddenly become charged with electricity of contrary descriptions at the points in communication with the earth (that is, through the protecting wires and apparatus at the extreme stations), causing phenomena such as those above indicated. The exterior charge, in flowing off immediately afterwards, produces contrary electric movement in the wires, which movement follows very closely the one first named, becoming confounded therewith, and in most cases neutralizing its effects. It is, therefore, only exceptionally that we have to record the influence of thunder storms upon subterranean telegraph lines.

As somewhat related to this subject, the following, from the proceedings of the Electrotechnic Society of Berlin, session of April 28th last, will prove interesting:

Professor Fœrster gave an account of some researches in cosmic and telluric electricity; he first cited the more recent experiments of Andries, Hoppe, Gerland and others on the origin of atmospheric electricity, and on the production of energetic tension, as shown in thunder storms. He then mentioned the theory of Edlund, respecting the influence of the rotation of the earth, considering the latter as a magnet, alluding also to the previous analogous ideas of Faraday, and finally to the opinions of William and Werner Siemens on the constitution of the sun and its electric action upon the earth. As proof and support of the latter views, the speaker cited the results obtained during the past five years in relation to the evidently electric character of a large portion of the phenomena observed in comets, and drew some conclusions therefrom permitting a better explanation of the peculiarities of the zodiacal light.

In conclusion, Professor Fœrster mentioned briefly the work of observation upon telluric currents made on German electric (subterranean) lines. It has been thereby proved that the slight, but regular, variations of these currents correspond exactly with the progressive movement of the earth considered as a magnet. These results are much more precise than those obtained at Greenwich, where lines quite short were used, and where the currents given by the terminal earth-plates showed relatively preponderating action.

BOOK NOTICES.

PROPERTIES OF MATTER. By P. G. Tait, M. A., Sec. R. S. E., etc. Edinburgh. Adam and Charles Black. 1885.

The advent of a new text-book on a subject which is now attracting much attention, is sure to be welcomed by students in this country, as well as abroad, as an opportune arrival and is assured of a cordial reception from all who are familiar with the author's admirable style and perspicuity of expression.

Professor Tait's name is especially familiar to us by his association with Sir William Thomson in the joint production some years ago of a now classic work on physics, in which the various theories of the nature of matter which have been advanced from time to time are clearly set forth.

The subject of this elementary work forms, as the author tells us in his preface, "the introduction to the course of Natural Philosophy in Edinburgh University." * * * "The work is (with the exception of a few isolated sections) intended for the average student, who is supposed to have a sound knowledge of ordinary geometry, and a moderate acquaintance with the elements of algebra and of trigonometry."

"But he is also supposed to have—what he can easily obtain from the simpler parts of the first two chapters of Thomson and Tait's *Elements of Natural Philosophy*, or from Clerk-Maxwell's excellent little treatise on *Matter and Motion*,—a general acquaintance with the fundamental principles of *Kinematics of a Point*, and of *Kinetics of a Particle*."

The volume is a small one, comprised in 315 pages, divided into fourteen chapters, with an appendix, as follows:

Chapter I, Introductory. II, Some hypotheses as to the ultimate structure of matter. III, Examples of terms in common use as applied to matter. IV, Time and space. V, Impenetrability, porosity, divisibility. VI, Inertia, mobility, centrifugal force. VII, Gravitation. VIII, Preliminary to deformability and elasticity. IX, Compressibility of gases and vapors. X, Compression of liquids. XI, Compressibility and rigidity of solids. XII, Cohesion and capillarity. XIII, Diffusion, osmose, transpiration, viscosity, etc. XIV, Aggregation of particles.

The appendix contains: I, Hypothesis as to the constitution of matter. By Professor Flint, D. D. II, Extracts from Clerk-Maxwell's article, "Atom." III, Vitruvius on Archimedes' experiment. IV, Singular passage of the "Principia."

The book is fairly, though not profusely illustrated with diagrams, and the mathematical formulæ are by no means formidable. Although dealing largely with such well known properties of matter as gravitation, inertia, divisibility, etc., the well-informed reader will find these threadbare subjects treated in a novel and forcible manner, and no one can peruse the book carefully without gaining valuable information on all the topics treated of.

It is to be regretted that the second chapter on the "Structure of Matter" is so very brief, and therefore unsatisfactory. A popular exposition of Sir Wm. Thomson's "Vortex Atom Theory," would have been eminently appropriate and advantageous, and the subject of his profound address at the Montreal meeting of the British Association for the Advancement of Science, entitled "Steps Toward a Kinetic Theory of Matter," is not even alluded to. It is safe to say that this address has done more to enlighten students in America, in regard to the modern conception of the nature of matter, than any other thesis. With this exception, there is little room for adverse criticism, and the volume, as a whole, is entitled to be called a model text-book, it will fill a hitherto vacant niche on the shelves of the student's library, and will be found a convenient book of reference to the teacher.

The author states in his preface that it is his present intention to continue his series of text-books by similar volumes on *Dynamics*, *Sound* and *Electricity*, it is to be hoped that he will shortly carry out this plan, and thus complete a work which has been so well commenced. A. E. O., Jr.